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THE NATURE OF THE ABSOLUTE JUDGMENT OF PITCH[1]

BY CARL H. WEDELL

Princeton University

I

'Absolute pitch' is the ability to name a tone that has just been heard without comparing its pitch to that of a standard tone. This ability has always been regarded by the musician as an exceptional power or gift, and many examples have been recorded in the literature of the remarkable feats of those who possessed it. Stumpf's discussion of Mozart's sense of pitch marks the beginning of psychological interest in the problem, and of efforts to determine accurately the factors involved in this judgment.

The present study is designed to ascertain whether unmusical persons can acquire the ability, to find out how accurate they can become, and to investigate the effect of different series of tones on the learning. The last problem will be explained more fully in the next section. For the present, we are interested in previous experimental attacks on this elusive gift held so much in awe by the musician.

Although v. Kries and others, fortified by the testimony of trained artists, had denied that absolute pitch could be acquired, Meyer and Heyfelder set out to test the statement by attempting to train themselves. Meyer (**6**) reports that with

[1] From the Psychological Laboratory of Princeton University.

the use of tuning forks, the percentages of correct judgments were 83, 78, 70, and 56, on 9, 11, 14, and 16 forks respectively. Likewise with notes of the piano, the percentage of correct judgments decreased as the number of notes increased. The results for Heyfelder were about the same. It is not clear why Meyer thinks that the influence of training is shown in the experiment, for it is quite evident that both subjects had a certain amount of training or ability to start with.

Köhler writes that after two weeks of practice on the white notes of the piano from C to b^3, 112 out of 220 judgments were correct. Since he does not state how accurate he was at the beginning of the experiment, it is not possible to tell how much benefit he derived from the practice. He maintains that his accuracy was due to paying especial attention to the 'tone-body.'

The effect of a long period of practice in recognizing notes is shown clearly, however, by the experiment of Gough (**4**), who trained 90 Smith college students on the piano. On the average, the group of subjects improved from an average error of 5.5 semitones on the first trial to 4.5 semitones on the last trial. The improvement was not uniform, however, with all subjects, and the curves show a gradation in ability from those who could identify correctly 83 out of 88 notes to those who could accurately name only one. The individual practice curves, which are given for only five subjects, give no evidence of a plateau at any point. They are extremely irregular, but show a decrease in the average error per trial as a result of practice. As for the distribution of errors over the keyboard, the fewest errors were made in the middle octave, the number increasing toward either end of the scale, except that the highest note and the lowest note had the fewest errors of all. The exact order of octaves from that with the smallest number of errors to that with the largest number was once-accented, twice-accented, small, thrice-accented, and great. This is a frequency distribution of errors over the scale similar to that which Stumpf (**9**), Baird (**2**), Weinert (**10**), Abraham (**1**), Whipple (**12**), and Petran (**8**) have found. These experimenters do not agree exactly on the order of octaves from easiest to

hardest, but they do agree that the middle regions of the scale are not as difficult, judging by the number of errors made, as the extremes. This is interesting in view of the results of the present experiment which are discussed below.

The data obtained by Mull (7) also show that practice has a definitely favorable effect on the ability to estimate pitch in an 'absolute' manner. She trained 12 subjects, but the most pertinent facts are those obtained from 'Group 1' which consisted of six persons who practiced identifying the pitch of middle c. This group gave judgments that were correct 40 percent of the time before training, with an average error of 285 cents (100 cents = 1 semitone). After training, they were correct 82 percent of the time with an average error of 33 cents. When the same group attempted to learn to select middle c from a scale consisting of nine tones, one separated from the next by only 8 ~/sec, their final performance was an average error of 29 cents. It is to be noted that decreasing the distance between tones in the scale did not change the size of the average error appreciably.

These experiments have established a few important facts. They have shown that a certain amount of absolute pitch ability can be acquired. Further, Gough's and Mull's work has indicated that the greatest amount of improvement takes place during the very first part of the learning period. The results of the former also have made it plain that in a large group of persons there are a few who possess the ability to a high degree, some who do not have it at all, and a great many who stand between these two extremes.

II

The Problem

It was the object of the experiment to determine whether the subject learns to recognize individual notes, or whether he learns the extent and character of a whole scale. Stated in a different manner the question is, does the observer learn a number of tones individually so that if given some other notes he would have to learn their pitches in turn? Or does he learn the position of the notes relative to a whole scale, so

that he could judge almost any number of pitches with the same degree of accuracy?

This problem has not gone unnoticed, it is true. Gough states, "In acquiring a memory for absolute tone the observer does not remember well one or several notes and the others not at all, but he builds up a more or less cohesive structure about a few or many notes." And again, "By repeatedly directing attention to the unity of the series, one may acquire the mental adjustment which finds little trouble in quickly assigning to random notes a position in a relatively well organized whole" (4, p. 92). Mull also writes, "Rather does it seem that, after a very rough initial placement of the notes as high, low, or intermediate, some feature in its relation to the total tonal configuration furnished the basis for judgment" (7, p. 490).

It is evident that some of the later experimenters are aware that even in absolute pitch, the judgment is at least in part one of relation. The quotations cited are all based upon introspective reports, and they could be balanced by as many quotations from the older literature to show that the observer learns to respond to an individual note. It is hoped that the results of the following investigation will be at least the beginning of an experimental solution of the problem.

III

Apparatus

In order to carry out the experiment, a means of producing a large number of relatively pure tones was required. The tones were generated electrically by a Western Electric 6010-B vacuum tube oscillator. With this instrument, it is possible to produce a tone of any frequency from 35 to 50,000 ∽/sec, and although the intensity of the overtones could not be measured, they were estimated to be about as prominent as in the tones produced by tuning forks. Since the ear is relatively less sensitive to the low frequencies, the overtones were more noticeable to the trained ear in that part of the scale.

The current was conducted from the oscillator through a potential attenuator and a two-stage, transformer-coupled amplifier to two Western Electric Type B, 552-W ear phones connected in series. The potential attenuator was a means of varying the tone from below the threshold to a painfully loud sensation.

Subject and experimenter communicated by means of a telephone since all the work was done with the observer in a sound proof room. Therefore, while the experiment was in progress, both E and O wore two headphones, one in which they heard

the stimulus tone, and one through which they received communication from each other.

The subject had before him a large cardboard chart on which were represented at appropriate points on a logarithmic scale the tones which he was learning. They were designated by numbers indicating their vibration rates.

The Subjects.—Seven persons in all served as subjects in the experiment, four in the first part and three in the second part. Three were graduate students, and four were members of the departmental staff. None was actively engaged in the field of music, nor did any play an instrument regularly. Ka, Ho, Mo, and Ba had never had any formal musical training. We had had only two years of piano training in early life. Hu had had about six years of piano training, and Br had had about thirteen years of rather intensive practice in playing the same instrument.

IV

Results

The experiment itself can be described most easily under two headings. In Part I the general procedure, the method peculiar to this part of the experiment, and the results obtained by this procedure will be discussed. In Part II, the method will be mentioned only insofar as it differs from Part I.

Part I

Procedure.—In order to rule out so far as possible the influence of intensity, the tones were all given at a loudness level equal to the loudness of a sound of 1000 ~/sec raised 40 db [2] in intensity above the threshold. The low frequencies do not have to be raised 40 db to sound equally loud with 1000 ~/sec at that level. The graph on p. 230 of Fletcher (3) was used to determine how much to increase the intensity of each tone above the threshold. Inasmuch as all of the subjects were practiced observers, a modified method of limits with six judgments with the tone increasing in intensity, and six with it decreasing in intensity were used to determine the threshold for each tone.[3]

As a further check upon intensity, during sessions 18, 19, and 20 for Ba and Ho; 17, 18, and 19 for Br; and 16, 17, and 18 for Mo, the loudness level of the tones was changed to

[2] The intensity of a sound in db = $10 \log_{10} I_x/I_o$ where I_x is the given physical intensity (say in ergs/sec) and I_o is the threshold intensity.

[3] Because of the fact that the output of the oscillator could not be measured in any absolute terms, this procedure involves the assumption that the curve of the threshold values for these Os was of the same form as that indicated on Fletcher's graph.

determine whether this characteristic was being used as a cue. In the first of these three sessions, all of the tones were raised 10 db in intensity. In the next, they were lowered 10 db, and in the following session, they were presented at haphazard values varying between 10 and 30 db from one another. It will be seen from the results shown in Fig. 1, where the asterisks indicate the point at which the intensity was changed, that the size of the average error was not affected.

The series of twenty-five tones used in Part I is shown in Table I under the heading 'Series 5.' The tones were spaced an approximately equal number of dl's apart. To obtain this number, the value of the threshold as given on p. 152 of Fletcher (3) was used. Since the size of the dl is continuously changing, the number of dl's between any two points could be accurately calculated only if the equation of the curve were available. As this equation is not known, the distance in terms of dl's between two tones as shown in Table I was obtained by a method of approximation.

The musical interval between the tones can be compared with the intervals on the piano by computing vibration ratios. Thus in Series 5, the interval is a minor sixth between the two lowest notes, and from there upward decreases gradually to about a minor third between the fourteenth and fifteenth, after which it remains constant to the upper end of the series.[4]

Each experimental session, of which there were three a week, consisted of one test repetition of this series, and three practice repetitions. The test presentation always came first each day. In this, the O was presented a tone for five seconds and then required to identify it. After a short interval of ten seconds or so, a second tone was presented. In this manner the whole series of twenty-five tones was given once. The practice repetitions which followed immediately were the same, except that the subject was corrected every time that he made a mistake.

The instructions to the subject were as follows:

[4] The standard piano range, with middle c tuned to 256∼/sec is from 26.6 to 4096∼/sec. So the scale used here had an upper and a lower limit about an octave higher than the piano.

TABLE I

SHOWING THE STIMULUS TONES USED, AND THE NUMBER OF DL'S BETWEEN ADJACENT TONES IN EACH SERIES

Series 1		Series 2		Series 3		Series 4		Series 5		Series 6		Series 6 (continued)	
⌣/sec	No. of dl's	⌣/sec	No. of dl's	⌣/sec	No. of dl's	⌣/sec	No. of dl's	⌣/sec	No. of dl's	⌣/sec	No. of dl's	⌣/sec	No. of dl's
50	343.6	50	173.2	50	108.7	50	80.7	50	51.1	50	25.4	1075	25.4
350	350.3	170	170.4	120	124.1	100	92.5	80	57.6	65	25.7	1160	27.6
1070	326.7	350	174.3	225	118.5	170	83.9	120	52.2	80	29.6	1260	27.9
2840	314.3	630	166.5	360	112.0	250	86.5	160	56.7	100	28.0	1370	30.5
7500		1040	169.1	530	112.2	350	83.4	210	58.3	120	26.8	1500	26.6
		1720	164.7	750	108.9	470	86.0	270	59.8	140	25.4	1620	27.7
Av.	333.7	2820	165.9	1040	110.9	620	84.2	340	51.7	160	29.7	1760	27.2
		4640	154.8	1450	110.1	800	84.2	410	58.1	185	27.0	1910	28.4
		7600		2010	110.4	1030	82.8	500	56.9	210	29.9	2080	27.5
				2800	109.7	1320	85.6	600	54.8	240	28.4	2260	27.0
		Av.	167.4	3890	108.1	1700	82.8	710	56.1	270	26.8	2450	27.4
				5400	101.3	2180	83.4	840	54.7	300	33.0	2660	28.9
				7500		2800	83.9	990	52.9	340	30.5	2900	26.5
						3600	83.8	1160	55.5	380	21.2	3140	29.0
				Av.	111.2	4630	80.8	1370	57.1	410	33.3	3425	26.6
						5950	76.2	1620	54.9	460	24.8	3710	29.2
						7640		1910	55.9	500	29.3	4050	26.1
								2260	54.4	550	27.6	4380	27.4
						Av.	83.8	2660	55.4	600	25.6	4770	26.2
								3140	55.6	650	29.1	5160	30.0
								3710	55.3	710	29.2	5650	26.7
								4380	53.6	775	26.9	6170	29.4
								5160	56.7	840	28.5	6800	25.6
								6170	55.0	915	26.2	7400	
								7400		990	27.5		
								Av.	55.4			Av.	27.7

"You will be presented a series of tones in haphazard order, and you are to identify each tone in terms of the number of double vibrations per second shown on the scale in front of you.

"Two seconds after the experimenter says 'ready,' he will present a tone for five seconds. As soon as the tone stops, you are to tell him which one it is.

"A certain number of tones each day will constitute a test series, during which you will not be corrected, but after that you will be corrected each time that you make a mistake.

"The object of this experiment is to see how quickly you can learn to recognize each tone correctly each time that it appears. Make every attempt to fix each tone in your mind in terms of its pitch."

A tone was repeated if the O desired it, but this did not happen more often than once each session, and although after-singing was allowed, the subjects were encouraged to respond quickly. Corrections were accepted only when they were made before the next tone was presented.

Thus the three practice repetitions each day constituted the learning period. A session took slightly less than an hour, and as there were 23 sessions for two of the subjects, and 22 and 21 for the other two, the Os spent three hours a week for over seven weeks on the experiment.

A haphazard order was used in presenting the tones, and this was arranged in such a manner that there were an equal number of large and small steps, but two adjacent tones were never presented following one another. Furthermore, the order was begun at a different place at each session so that the same tone never commenced either a test or a practice series twice. The order was the same for every session throughout Part I except that after the 16th session for Ba and Ho, and the 15th and 14th for Br and Mo respectively, it was changed to ascertain whether the subjects had memorized it. The new order can be seen on Fig. 3 under the heading 'Series 5.' That this change had no effect on the results can be seen in Fig. 1, where the small cross lines on the graph indicate the point at which the new order was introduced. The average error of two of the subjects is less following the alter-

ation of order, while that of the other two is greater. It would have been almost impossible for them to have memorized the order in the test repetition anyway, because they never knew whether they were correct.

In the next to the last session, nine tones were replaced, unknown to the subjects, with tones near them in frequency, and in the last session, the entire scale of tones was shifted slightly. In Table II, the original 25 tones (Series 5) are given

TABLE II

SHOWING VIBRATION FREQUENCIES OF ORIGINAL 25 TONES, AND OF NEW TONES SUBSTITUTED DURING THE LAST TWO SESSIONS OF PART I

1 Original 25	2 9 New Tones	3 25 New Tones	4 Column 1– Column 3
⌣/sec	⌣/sec	⌣/sec	semitones
50	50	40	4
80	**70**	70	2⅓
120	120	135	2
160	160	180	2
210	**200**	200	1
270	270	280	⅝
340	**320**	320	1
410	**380**	380	1⅓
500	500	540	1⅓
600	600	620	⅝
710	**740**	740	¾
840	840	800	⅝
990	990	1000	⅕
1160	**1100**	1100	1
1370	1370	1400	⅜
1620	1620	1720	1
1910	**1860**	1860	½
2260	2260	2200	½
2660	2660	2750	1
3140	**3200**	3200	⅓
3710	3710	3600	½
4380	4380	4250	½
5160	**5000**	5000	½
6170	6170	6400	⅗
7400	7400	7000	1

in the first column, the nine new ones are shown in boldface type in the second column, in the third column the new series is given, while the fourth column indicates the difference in semitones between the old and new series.

The final procedure in Part I was the relearning of the tones after an interval of three months. Then two test

presentations were obtained with a series of 49 tones, consisting of the former 25 plus 24 new ones whose frequencies were just halfway between each pair of the original 25. This series, together with the number of dl's between adjacent tones is shown as 'Series 6' in Table I.

Results.—Figure 1 gives the results for the four subjects for the test repetitions of each session. The abscissa gives the number of the session, and the ordinate gives the average error in dl's for the session indicated on the abscissa.

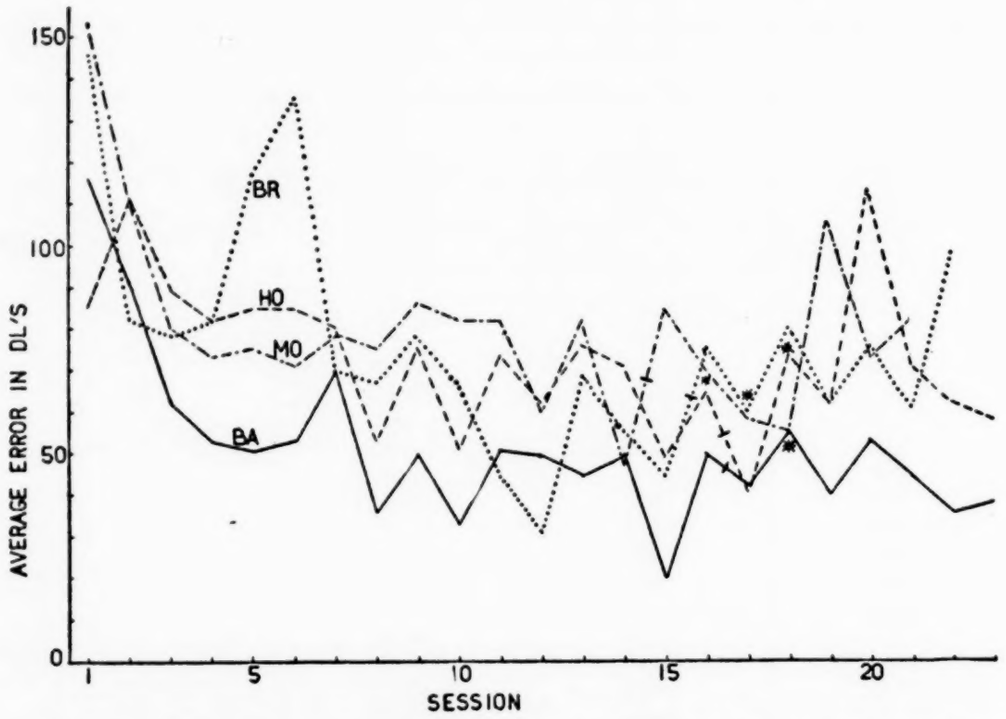

FIG. 1. The effect of practice on the ability to recognize pitch. Short cross lines indicate where order of presentation was changed, and asterisks indicate first of three sessions in which intensity was varied.

The curves are quite typical of learning curves, since they show the sharp descent during the first few sessions, and the more gradual slope later. By the fourth session all Os had made their large gains, and by the eighth all had reached a level of performance around which they varied during the rest of the experiment. It is impossible to tell whether longer and more concentrated effort would have enabled them to do still better, but they seemed to be on a plateau, so the experiment was discontinued. The sudden increase in errors made by

the two subjects in the 19th and 20th sessions is probably due to fatigue or some emotional disturbance.

A sharper initial drop, and a definite plateau, are the chief differences between these curves and those given by Gough (4). There is no doubt, however, that the subjects did cut their errors about in half as a result of the practice.

The curves of Fig. 1 show a great irregularity for the individual subject. Some of this was caused by a wavering interest in the problem, and a consequent variation in effort. This is especially true of the slight tendency of the curves to rise in the last few sessions.

Distribution of Errors by Tones.—One of the noticeable things about all of the previous work in absolute pitch is that the fewest errors were made in the middle regions of the scale, with the exception of the end tones in some cases. Yet in the present experiment almost opposite results were obtained. Fig. 2 gives the average error per tone in terms of dl's for the

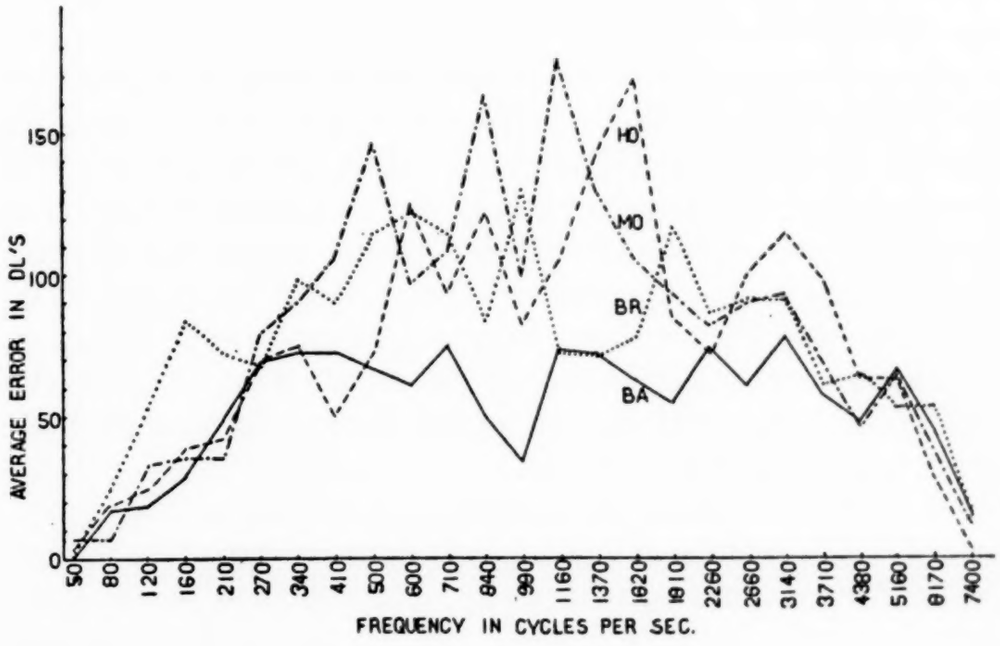

FIG. 2. Average error per tone.

test repetitions of each session of Part I. In spite of the irregularity of the curves, a general rise is apparent from each end toward the middle, with the middle more or less level from 500 to 1910 ~/sec.

The fact that there is no tone upon which all of the Os did better than upon any other is a fairly good indication that there were no constant differential cues. One or two of the tones did develop an attendant noise in a session, and if this remained constant throughout a session, it was immediately seized upon by the Os. So that had it continued, it would certainly have been evident in the results shown in Fig. 2.

The subjects were unanimous in declaring that there was considerable qualitative difference between notes in various regions of the scale. The lowest note was a flutter or rumble, the next higher ones had less of the rumble and more of a buzzing quality; from about 160 to 710~/sec was a region which seemed to have a unity, while from there up to 3710~/sec the tones were thought of as being 'somewhere in the middle.' But at the upper end of the scale a shrill, sharp quality was distinguished. These observed differences may have been due in part to the purity of the tones. The overtones in an instrument like the piano have the effect of somewhat concealing pitch differences.

In an experiment of this type, the O presumably makes his judgment on the basis of some scale that he carries within himself (*cf.* Wever and Zener, 11). Also, this subjective scale has some point or points of reference. Probably it is at these places that the O makes the fewest errors. With the subjects in this experiment, the ends of the scale served as points of reference, and they seldom made a mistake in judging them. The Os agreed that the middle of the scale was by far the hardest to judge, and this testimony is corroborated by their performance as seen in Fig. 2.

Just why previous experimenters have found the fewest errors in the middle of the scale, it is difficult to tell. With experienced musicians, this might be explained as due to their greater familiarity with that region (*cf.* Petran **8**, p. 27 and p. 116), but Gough's results can not be explained on this basis since she used inexperienced observers. However, some of her subjects started practicing the middle octave, and the others who were free to use their own method may have done the same thing.

There is in the data no evidence of a consistent tendency to overestimate or underestimate on the part of any one subject, or of all of the subjects taken together. Neither do the results indicate that the higher tones were underestimated, and the lower tones overestimated, as Mull (7) and others have found. (But *cf.* Petran **8**, p. 111).

It has been said that no judgment is "absolute" unless a long time, say 12 hours, has intervened since the last hearing of a tone. Following this line of reasoning in the present experiment, the only absolute judgment would be the first one each day. It must be admitted that there is some basis for this assertion, because some of the subjects seemed to compare the notes with one another deliberately. In addition to this, a subject would sometimes correct a previous judgment by saying, "Oh, that other one must have been [so and so]."

Table III gives the error for the first judgment each day in the test repetitions. The average error shown at the foot of each column of this table is of about the same order, except for the subject Mo, as those shown in Fig. 1. There is no indication that the first judgment differed from the rest. But as Petran (**8**, p. 43) has pointed out, this does not preclude the possibility that the first tone judgments were absolute and the following ones relative, since especially with untrained observers a relative judgment may be wrong as well as right.

To investigate the effect on the learning curve of the substitution of new tones for the more familiar ones, nine tones differing slightly from some of those in Series 5 were substituted in the next to the last session while the other 16 remained the same. Then in the last session, an entirely new series was given as shown in Table II. The Os had to make their responses on the basis of the old chart, and were not told beforehand that new tones had been introduced. The average error for these two trials or sessions is given in the last two points on each curve of Fig. 1. A glance at the curve shows that these figures are of the same order of magnitude as those of the last eight or nine sessions. Thus the introduc-

TABLE III

SHOWING THE ERROR IN TERMS OF DL'S ON THE FIRST TONE PRESENTED EACH DAY, PART I

Tone	Subject			
∽/sec	Ba	Ho	Mo	Br
500	58	112	228	168
1160	113	53	278	0
410	169	52	170	52
210	57	0	0	118
1910	110	56	166	56
1620	55	276	111	221
160	52	110	52	57
4380	111	0	55	111
840	56	55	111	56
3710	0	0	109	0
270	115	58	167	60
7400	55	0	0	0
2260	54	110	221	110
710	112	111	55	56
340	0	0	60	0
6170	57	110	0	221
600	57	0	285	167
990	0	108	53	220
2660	0	55	0	54
80	52	0	—	52
120	0	109	—	—
Average	61.1	65.5	111.6	89.0

tion of new tones displaced anywhere from 1/5 to 4 semitones from those with which they were familiar did not disturb them at all. However, one of the subjects (Ba) did recognize that new tones had been introduced, and one other thought that 50∽/sec sounded queer. In both cases, this was the result of the large difference between 50 and 40∽/sec. The tones did not sound unfamiliar enough to the others to elicit any comment.

After an interval of two months for three of the Os, and four months for the other (Br), they were all retrained on the old tones until their average error per session was the same as it had been when they stopped learning. This took only two sessions with three of the subjects, and three sessions with the other (Ba). They were then given two test repetitions of a 49 tone series made up of the old 25 plus 24 new ones (Series 6, Table I). The repetitions were separated by an

interval of 48 hours. The subjects had before them a new chart with the tones indicated by their vibration rates at appropriate points on a logarithmic scale.

Table IV shows the average error for each of the four subjects at the two sessions. Three rows of figures are presented for each subject so that the error made on the series as a

TABLE IV

SHOWING THE AVERAGE ERROR IN DL'S MADE IN THE TWO SESSIONS ON SERIES 6; AS A WHOLE (ALL 49), ON THE PREVIOUSLY LEARNED TONES (OLD 25), AND ON THE NEW TONES (NEW 24)

Subject	Tones	Session	
		1	2
Ba.....	All 49	52.4	63.2
	Old 25	51.5	48.2
	New 24	53.5	79.8
Ho.....	All 49	64.0	71.2
	Old 25	57.9	69.7
	New 24	70.4	72.6
Br.....	All 49	61.5	63.2
	Old 25	54.7	50.4
	New 24	69.2	79.5
Mo.....	All 49	108.0	96.1
	Old 25	104.0	112.6
	New 24	113.0	74.9

whole can be compared with that made on the original 25 tones in this series and on the new 24. Comparing the average error on the 49 tones (Table IV) with that made during the last ten or eleven sessions on Series 5 (Fig. 1), we see that only one of the subjects (Mo) did worse on the 49 tones than he had been doing during the latter part of the training on the 25 tone series.

When we look at Table IV and compare the average error on the more familiar tones with that on the less familiar, we find that in seven out of eight cases it is larger for the new tones. Only in the case of Ba on the second session is the error significantly larger on the new tones. That is, only in this case is the average error definitely outside the range of variation of previous sessions as given in Fig. 1.

Summing up then, we find that three out of four Os were not appreciably disturbed by being presented a series of tones in which the distance between any two was just half that which they had become accustomed to. Moreover, they made nearly as many errors in judging the more familiar tones in such a series as in judging those which were strange. This can only mean that in this experiment, the training had the effect of establishing a familiarity with an auditory range. It did not serve to establish a response to specific tones. This is seen more clearly in Part II which was designed to investigate the phenomenon more thoroughly.

Part II

Procedure.—The method devised was as follows. Three Os learned first a series of five tones (Series 1, Table I) which were an approximately equal number of dl's apart, and covered the same range of frequency as Series 5. Next, they practiced nine tones (Series 2), then thirteen (Series 3), and seventeen (Series 4). Lastly they had two test repetitions, separated by 48 hours, of the same series (Series 5) as was used in Part I. A series was considered learned when an O had made one errorless test repetition, but during the learning of the 13 and 17 tone series, it was necessary to advance some of the subjects to the learning of the next longer one before they had been able to satisfy this requirement. Otherwise, the experiment would have taken more time than was available. However, a subject was not allowed to proceed until he seemed to be making no further progress with the series at hand.

Another change in method in Part II was that throughout there were only two practice repetitions per session instead of three.

Results.—The results of using this method are shown in Fig. 3. The graph is a combined one giving the data for the three Os on all five series, for the test repetition of each session. The abscissa gives the number of the session, and the ordinate the magnitude of average error in dl's. Above each set of curves the tones are shown in the haphazard order in which they were presented.

It is evident that only two or three sessions are necessary for a subject to learn to identify tones as far apart as those in Series 1. The addition of four more makes the problem a great deal more difficult. However, one O had mastered it in five sessions, and the other two in seven and eight, respectively.

When the thirteen tone series is considered, it can be seen that it has really become a difficult task to assign the correct

vibration number to the tone. None of the subjects mastered the problem, even though one of them was allowed 13 sessions.

We was given two, Ka and Hu were given three complete sessions (test repetition and practice repetitions) on the 17 tone scale (Series 4). Their performance shows that one of them (We) did much better than he had on the previous series, while the other two remained the same.

As to Series 5, on which they were given two test repetitions, one subject (Hu) again reached a level of performance

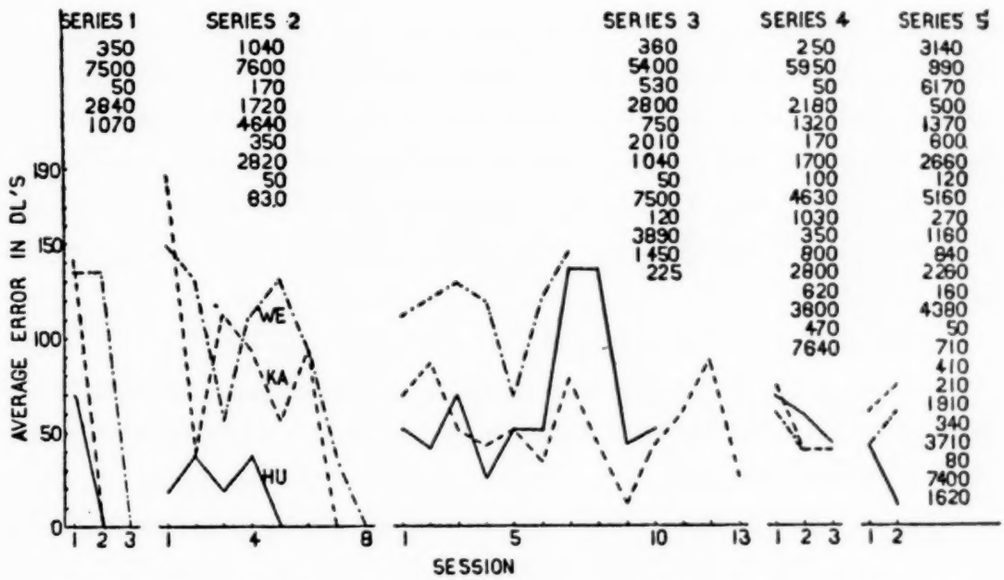

FIG. 3. The effect of number of tones in the series on pitch recognition ability. Above each set of curves is shown the corresponding series of tones in order of presentation.

he had formerly attained in Series 2, and the other two subjects stayed remarkably close to their previous performance.

At the end of this long training period, during which the distance between adjacent tones had become smaller and smaller, the Os were making an average error of between 12 and 70 times the limen, with the median for the three Os being around 50. This compares very favorably with the record made by the subjects in Part I. The best of the latter was averaging about 45, and the worst about 70 when the training was discontinued. This means that the former method results in as good a final performance as the latter.

In order to compare the results obtained in this experiment with those reported by others, it is necessary to translate the error into semitones. Throughout most of the tonal scale, a semitone contains about 20 dl's. As the Os at the conclusion of both parts of the experiment were making an average error of about 50 dl's, this means an error of 2½ to 3 semitones. Petran (**8**, p. 106) reports the same error in his experiment on the testing of musically trained reactors, and it is somewhat less than that given by Gough (**4**, p. 37 ff). The individual practice curves in both this experiment and that of Gough are so irregular that such an average is not very reliable. Yet the considerable agreement with diverse methods of attacking the problem may mean that an average error of from three to four semitones is the normal limit of ability in this problem, and that any considerable advance beyond this limit can only be attained by a few persons after most intensive practice.

The striking thing about the experiments of Part II is the fact that after Series 3 had been reached, the average error was not increased by increasing the number of tones between two limiting frequencies. This points to the fact, as suggested in Part I, that the person who acquires a pitch-naming ability is really acquiring a knowledge of the limits and characteristics of an auditory extent, rather than of individual notes within that extent. Otherwise the errors should have increased as each new series was introduced.

VI

Conclusions

1. Relatively unmusical observers can learn to increase their accuracy in assigning pitch numbers to pure tones.

2. The greatest increase in ability takes place during the first few practice sessions.

3. The limit of ability reached in this experiment was an average error of about three semitones.

4. The course of the learning process is very irregular, and there are large individual differences.

5. Unmusical observers can learn accurately and easily to recognize tones that are eight and one third semitones apart, but they fail to learn to judge the tones correctly when the interval is decreased to five and one half semitones or less.

6. Observers build up a subjective scale in which they can place unfamiliar tones as accurately as familiar ones.

7. Contrary to previous experimental results, the greatest average error was made in identifying tones from the middle of the scale, the size of the error gradually decreasing toward the ends.

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MODIFICATION OF THE GALVANIC REFLEX BY DAILY REPETITION OF A STIMULUS

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In spite of the interest and importance of the question of the acquisition, modification, and loss of emotional reactions, there has been surprisingly little experimental work concerned with the topic. H. B. English (**12**) remarked:

"Upon the pioneer experiments of Watson and Rayner on the conditioned emotional response there has been erected a towering edifice of theory; the factual basis for the edifice remains appallingly slight."

There is practical agreement among writers on the general question that important modifications of emotional reactions are brought about by the experience of the individual, some, Watson (**27**) and Allport (**1**) among them, explaining nearly all adult emotional behavior of the normal individual as acquired so, and others, Gesell (**13**) and H. E. Jones (**16**) for example, believe that certain emotional elements develop independently of the experience of the individual. Writers of both persuasions regard modifications of emotional reactions through experience as examples of conditioning, (Watson (**27**), Allport (**1**), Jones and Jones (**16**), Woodworth (**29**), Carr (**4**),and others). Certain principles of modifications derived chiefly from casual observation and from the Russian studies of the salivary reflex are extended to cover the development of emotional reactions. But there are difficulties in their generalization which are often overlooked, although they are fairly obvious. For example, mere coincidence of two stimuli sometimes apparently effects the transfer or extension of an emotional reaction [1] from one stimulus to another, but such a result is not universal; in fact when it occurs we are inclined to regard it as an abnormality. Again there are cases of emotional reactions which do not appear to become

[1] In this article the term is intended to include feelings.

extinguished when the stimulus is repeated: such as phobias, maternal reactions to a baby, and the sex emotions of courting couples.

H. E. and M. C. Jones (**17**) have recently summarized what literature there is concerning the experimental development of emotional reactions in children. The classic observations of Watson and Rayner were made on the development of a conditioned fear response in one infant. They observed that there was a generalization of the stimulus after the reaction was once formed. The means used for determining the presence of an emotional reaction was observation of overt behavior. Moss used the same method and observed the same phenomena of spread in his conditioning of two children to give an avoidance reaction to the sound of a snapper. M. C. Jones used general observation to study the elimination of a fear reaction by various methods in one child. Direct counter conditioning and imitation were found to be most successful in eliminating the reaction. English reports two cases in which a fear reaction was attached to an otherwise indifferent stimulus. He believes the process is not necessarily conditioning in the ordinary sense of the term. H. E. Jones reports the formation of conditioned galvanic reflexes in several children. This investigation is apparently the only one using instrumental techniques to study the development of emotional reactions in children, and one of the few cases of their use in any such study. The data reported are those for one child, and the author regrets that these are only qualitative. On the basis of these, however, he is inclined to think a number of principles of conditioning such as extinction, inhibition and spread of inhibition hold for emotional reactions.

Watson (**26**) reported no success in an attempt to condition the galvanic reflex in adults. Slight (**24**), however, found the reflex could be conditioned. The present writer (**6**) reported the influence of one stimulation upon the reaction to another, regarded by some as the initial stage of conditioning. In another study decrement was found in the reaction

to a 'primary' stimulus to the galvanic reaction when the stimulus was continued during one sitting.

Beebe-Center (**2**) summarizes a number of experiments concerned with hedonic habituation. Meyer (**20**) and Gilliland and Moore (**14**) and Downey and Knapp (**11**) found that the pleasantness of musical selection increased with intermittent repetitions. Martin (**19**) found the same of pictures. On the other hand, Young (**30**), and Herring (**15**) found a tendency toward indifference with various types of stimuli. C. W. Valentine (**25**) found little shift in the pleasantness of musical intervals.

Beebe-Center concludes:

> "There is, then, no generally valid law of hedonic habituation: it is not true that hedonic value shifts towards indifference *as a function* of intermittent repetition. Both with respect to relative and to absolute habituation, however, we saw that intermittent repetition is sometimes *accompanied* by a shift of hedonic value towards indifference."

It is the purpose of the experiments here reported to record the electrical skin resistance and galvanic reflex to stimuli of various kinds, repeated usually at intervals of one day, to determine whether a general principle of habituation is applicable, and so far as possible what particular principles are applicable to each type of stimulus. For convenience the variables investigated, galvanic reflex and electrical skin resistance are referred to as emotional reactions (the latter, however, only when its reactional character can be demonstrated) not because it is meant to imply that they accompany an affective mental element, but because it is the writer's conviction that they are entitled to that designation in their own right.

Some general term is needed to denote these and other physiological reactions to stimuli, and 'emotional reaction' seems to be satisfactory, as it has only very vague previous attachments. Of course, the relation between 'emotional reactions' and the heterogeneous phenomena that have been called emotions will remain to be demonstrated by experiment. It is notable, however, that practically all 'theories of emotion' make some place for these reactions here designated

as emotional. Hence experiment upon them should be expected to yield some information of general importance.

The measuring apparatus used for skin resistance and galvanic reflex was that described by Davis and Porter (8), in which the flow of current through the subject is kept constant by an electronic device and the resistance of the subject at any moment is determinable from the reading of a voltmeter and of a milliameter which is controlled by the potential difference across the subject fed into a direct coupled amplifier. Electrodes were of the usual Zn-$ZnSO_4$-NaCl type; the tips of the first two fingers were immersed in the NaCl. To insure that the same area of a subject was in contact with the electrolyte at each sitting, adhesive tape was wrapped around the fingers at the top of the nail. Such a method as this, of course, allows comparison of a subject's condition with his own previous or subsequent condition but does not permit comparison of the absolute value of the resistance of one subject with that of another. The current used was 0.1 milliampere in all cases.

The general method of recording was to take readings of the subject's resistance at certain prescribed intervals, one fourth, one half, or one minute according to the requirements of the situation. As a galvanic reflex consists of a decrease in resistance the occurrence of one would have the effect of lowering the next resistance reading. Obviously this method omits from consideration reflexes so small that complete recovery can occur within the recording interval, and records the minimum peak of only those reflexes which have exhibited no recovery during the recording interval. For the purpose in hand, however, the information obtained is sufficient, for it makes possible a statement of the average condition of a group of subjects at a series of given moments, whereas knowledge of a series of minima scattered irregularly in time, which the ordinary technique of recording deflections yields, would provide no means whatever of grouping data unless approximate temporal coincidence of deflections in different subjects can be insured. To supplement this regular procedure readings of minima were taken whenever the nature of the stimulating situation led to the expectation that nearly coincident minima would occur.

No reading was taken until current had been flowing through the subject for one minute. To obtain the value of the resistance at the moment the current is turned on, which seems to be of no particular psychological significance anyway, would require a photographic record from an oscillograph, because the rate of change is so great. Change has not ceased at the end of one minute (probably it will never cease in the average of a group of subjects) but at that time change is slow enough so that slight errors in the time of the reading make no considerable difference in the value of the resistance. For convenience this value is spoken of as 'initial resistance.' For a definite period after this initial reading, 5.5 minutes in most experiments, no stimuli were delivered to the subject by the experimenter. Readings were taken regularly during this period. The purpose of this interval was to study response to the general situation, and to avoid as far as possible confusion of this response with response to controlled stimuli.

In all experiments, the subject was seated in an arm chair in a booth of sheet metal with a screen top. He was unable to see the experimenter, the recording or stimulating apparatus. In experiments during summer months an electric fan was kept blowing into the subject's booth to reduce the discomfort from heat as much as possible. Even so, in one experiment, as will be pointed out, there is an apparent effect of temperature change during the week.

Experiment I.—The Effect of Daily Repetition of a Loud Sound

The stimulating sound was produced by a 1,000 cycle tuning fork oscillator (General Radio Co.) the output of which was far from pure. The oscillator was 'sound proofed' by enclosure in two boxes with an air chamber between, the outer box being covered inside and outside with layers of cane board. The box was located twenty feet from the subject, but the sound was still discernible with favorable attentional set. To avoid stimulating the subject by turning it on the oscillator was kept running all the time. The maximum electrical output of this oscillator was fed into a two stage Loftin-White amplifier (Electrad Co.), and the output of the amplifier was fed directly to a pair of 1,000 ohm earphones. A switch in this circuit, shunted with a 0.5 mfd. condenser, was used to start and stop the stimulus. The earphones were located on the roof of the subject's booth, about two feet directly above the top of his head. The sound produced by them was distinctly unpleasant because of its loudness and roughness.

Subjects were instructed that they would have a seven-minute period of rest, that the end of this fore period would be indicated by the signal, 'Ready,' from the experimenter, and that fifteen seconds after the signal the noise would begin and continue two minutes. The ready signal was thought advisable to keep the subject as much as possible from exciting himself with anticipation of the momentary onset of the noise. Readings were taken at intervals of one minute during the first five minutes, and at half minutes thereafter. In addition, readings of minima were obtained after the ready signal and after the onset of the noise. Eight subjects, three women and five men, continued the experiment for five consecutive daily sittings, for the most part at the same hour each day. Temperature during the week was fairly constant.

Readings were converted into ohms of apparent resistance and the average of the eight cases computed for each reading time for each day. These averages are presented in Table I, and graphically in Fig. 1. For the purpose of graphic repre-

TABLE I

EXPERIMENT I

Minutes	Averages of Resistances During Successive Sittings				
	Av. First Day	Av. Second Day	Av. Third Day	Av. Fourth Day	Av. Fifth Day
1	27,843	31,516	31,581	32,674	38,971
2	25,538	28,563	27,191	29,779	33,321
3	23,709	26,618	25,492	27,829	30,106
4	23,628	24,755	25,281	25,931	28,341
5	23,184	23,863	24,098	24,898	26,616
5.5	22,844	23,224	24,116	24,778	26,114
6	22,551	23,117	24,136	24,587	25,341
6.5	22,224	23,311	24,322	24,589	24,320
(Ready) 6.75	22,629	23,166	24,112	24,278	23,676
Min.	21,294	22,306	23,573	23,697	23,335
7 (Noise)	21,569	22,476	23,739	23,814	23,152
Min.	19,335	21,529	23,027	22,766	22,589
7.25	19,181	21,946	23,514	23,114	22,744
7.5	19,161	21,938	23,706	23,093	22,783
8.0	19,050	21,950	23,034	23,191	22,663
8.5	19,672	21,819	22,999	23,344	22,576
9	19,669	21,829	22,704	23,241	22,476

sentation the minima after the ready signal and after the onset of noise were plotted as though they occurred 7.5 seconds after the stimulus. The average resistance of the group never, of course, reached such a low point at any given moment.

The general configuration of these resistance curves resembles that previously reported for one sitting (Davis (**5**)). In all but one case the lowest point of the curve is immediately after the onset of the noise. There is indication of some recovery during the noise stimulation, but the period was not continued long enough to obtain the amount of recovery found in the study just referred to.

There are two prominent tendencies indicated by the successive curves in Fig. 1. First the size of the reactions to the ready signal and to the onset of noise becomes progressively smaller from day to day. Second, the absolute level of each day's curve is higher than the preceding days. These two tendencies are represented singly in Fig. 2 and Fig. 3. The difference between the resistance just before the stimulus, and the minimum just after (average galvanic reflex) was taken as a measure of the effect of the stimulus. Fig. 2 shows that this

value decreased in the five days to approximately one-fourth of its original magnitude. If the change were expressed as a percentage of the total resistance it is evident that the trend would be even stronger. It is notable that the curve for

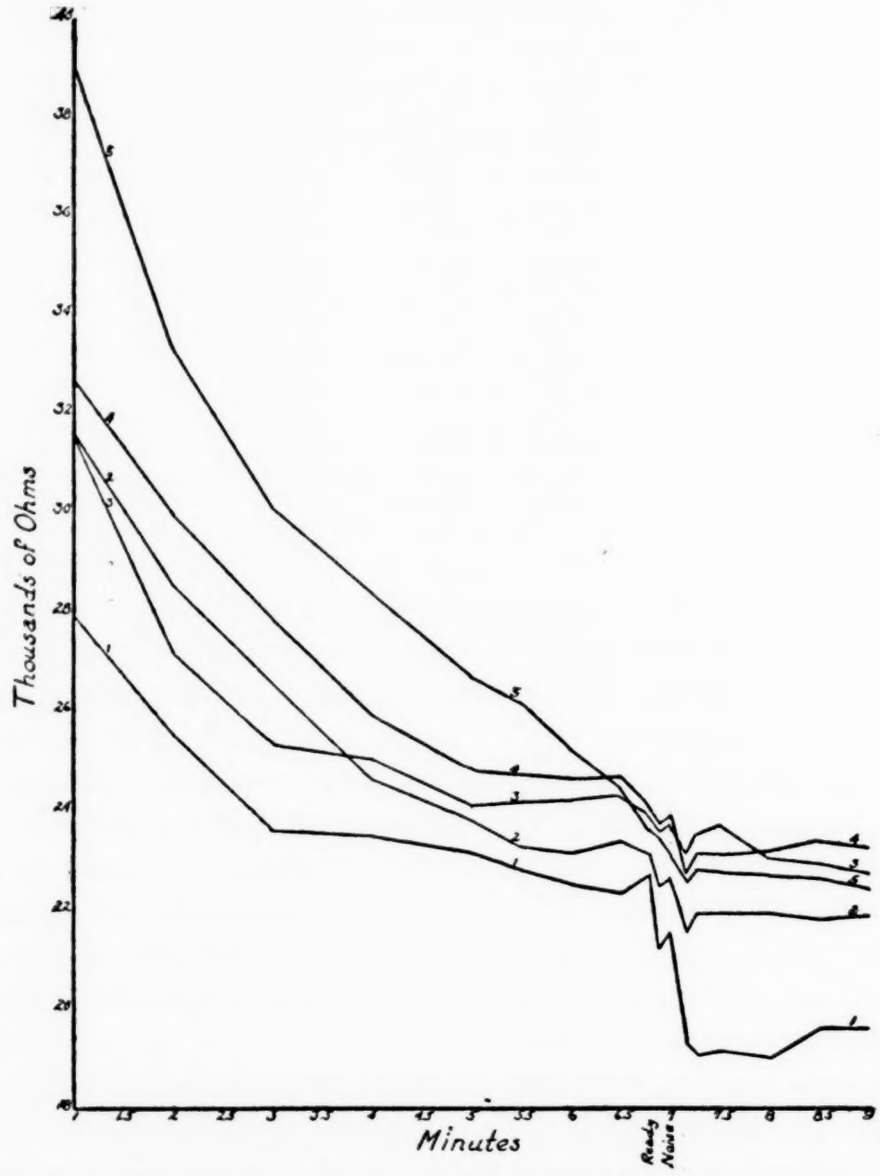

FIG. 1. Exp. I. Course of average resistance during successive sittings.

response to ready, largely a 'conditioned' stimulus, resembles the curve for reaction to noise, essentially a 'primary' stimulus.

A similar tendency for reactions to decrease upon repetition of stimuli was reported by Blatz (3). The stimulus in

his study was the collapse of the subject's chair. Measurements were made of the e.m.f. rather than the resistance changes in the subjects, and since this may fluctuate in either direction the findings are somewhat hard to interpret.

The initial resistance is taken as an index of absolute level. The average is plotted in Fig. 3. An increase of nearly fifty per cent is to be observed during the five days. The interpretation which the writer suggests is that the same process of habituation is operating here as in the reactions to warning

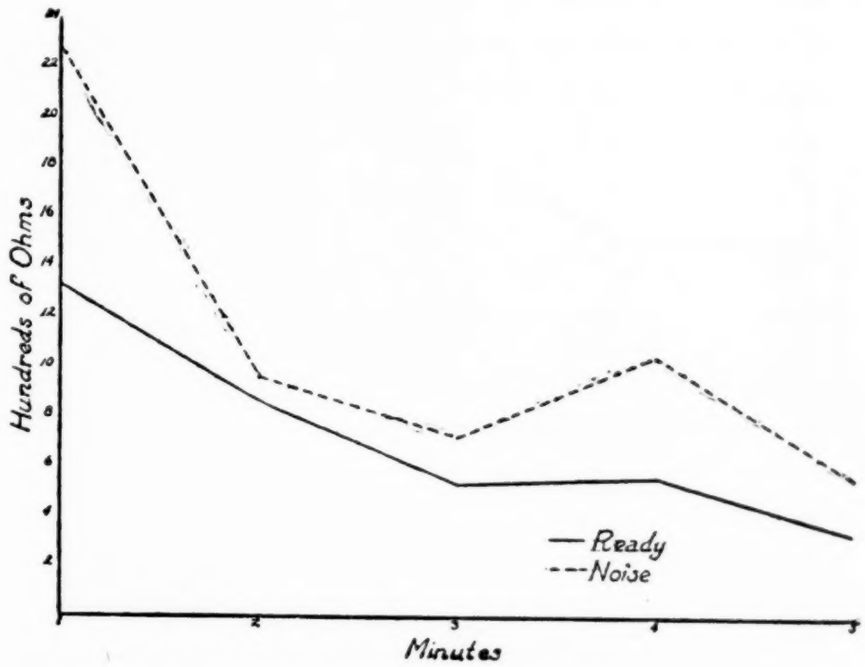

FIG. 2. Exp. I. Course of reflexes.

signal and noise. The experimental situation itself would be expected to have considerable stimulating value at first. A drop somewhat similar to that observed after the noise would have occurred in the subjects previous to the beginning of the sitting and the first reading would fall in a low part of the curve for that reaction. As the experiment was repeated, reaction to the general situation decreased, as did the reaction to the noise. If this interpretation is correct (and data from the remaining experiments tend to confirm it), the general level of resistance is hereby shown to be of psychological

significance, as has been already suspected by a number of writers.

In an experiment previously reported by the writer (7) a similar conclusion was reached from data showing the opposite trend under reversed circumstances. Subjects in their first sittings were given no stimuli, and anticipated none. In their second sitting a week later they were expecting a shock at a specified time. Initial resistance (from the back of the wrists) was substantially less in the second sitting.

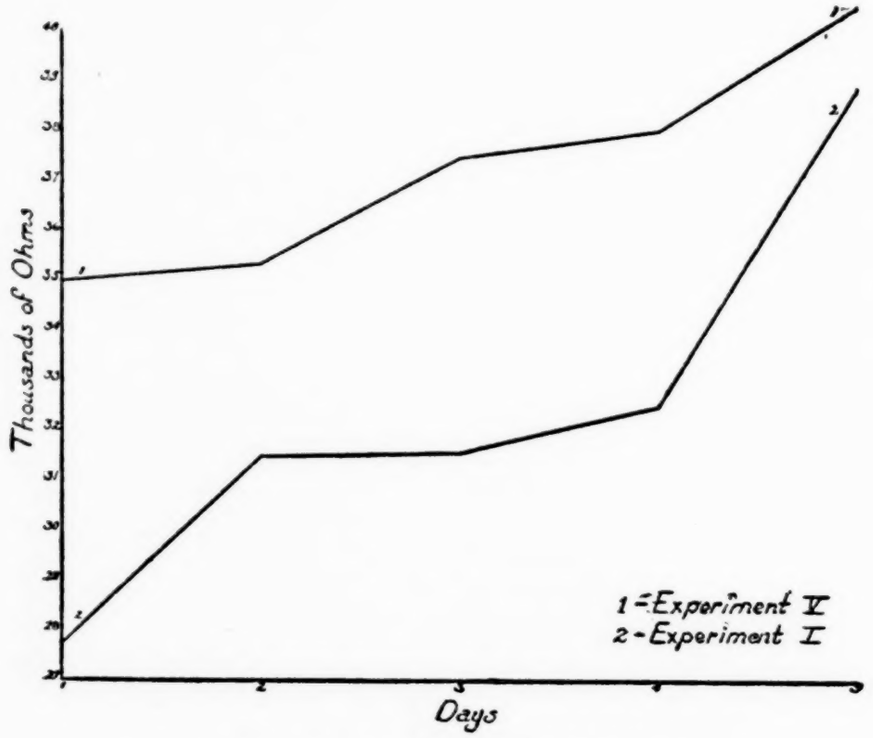

FIG. 3. Exp. V and I. Course of initial resistance.

EXPERIMENT II.—RECOVERY FROM HABITUATION TO NOISE

This experiment was designed to discover how long the effects of the 'training' given in the first experiment would last. It was possible to test six of the subjects used in the first experiment after lapses of considerable intervals. The same procedure was used with these subjects on the sixth and on the thirty-second days after the end of the original 'training' series. Table II shows the averages of the six on the first and last days of the training series, and on the two

TABLE II

EXPERIMENT II

Average Initial Resistance and Reflexes Before and After Training and Period Without Training

	1st Day	5th Day (End of Training)	11th Day (1st Retest)	39th Day (Second Retest)
Initial Resistance.....	27,923	38,748	35,082	35,543
'Ready' Reflex.....	1,243	356	562	856
'Noise' Reflex.....	1,096	647	691	1,051

tests after intervals. Magnitude of response to the ready signal and to the noise were computed as before; they are represented in Fig. 4. A graph of initial resistance is presented in Fig. 5.

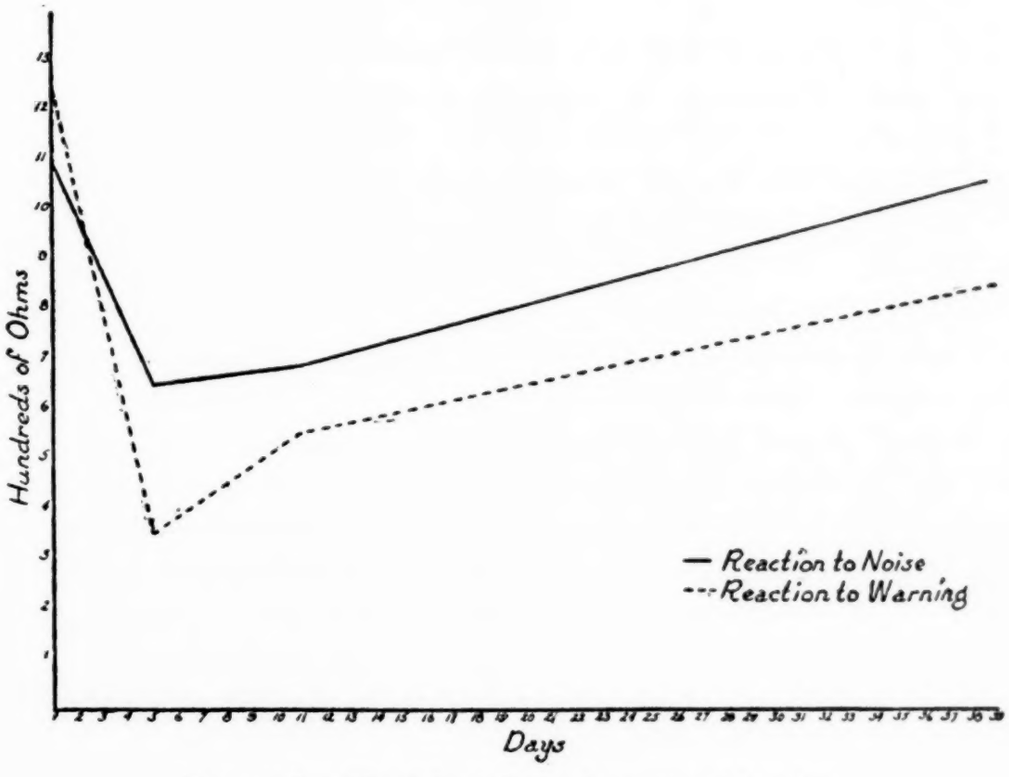

FIG. 4. Exp. II. Adaptation and recovery of function.

It may be observed that the reflexes to the warning signal and to the noise show similar amounts of recovery on the tenth day (after six days of rest) and by the thirty-eighth day have returned to a level not quite so high as at the beginning of the

training series. The curve of initial resistance shows much less effect of 'recovery' from the effects of the daily stimulation. In other words, the habituation of the subjects produces a more permanent effect on the measure. Following the suggested interpretation of resistance level this persistence of effect implies that rate recovery of reaction depends upon the nature of the stimulus. The general experimental situation is of course much more complex than either of the other two stimuli. The noise stimulus is a 'primary' one and the warn-

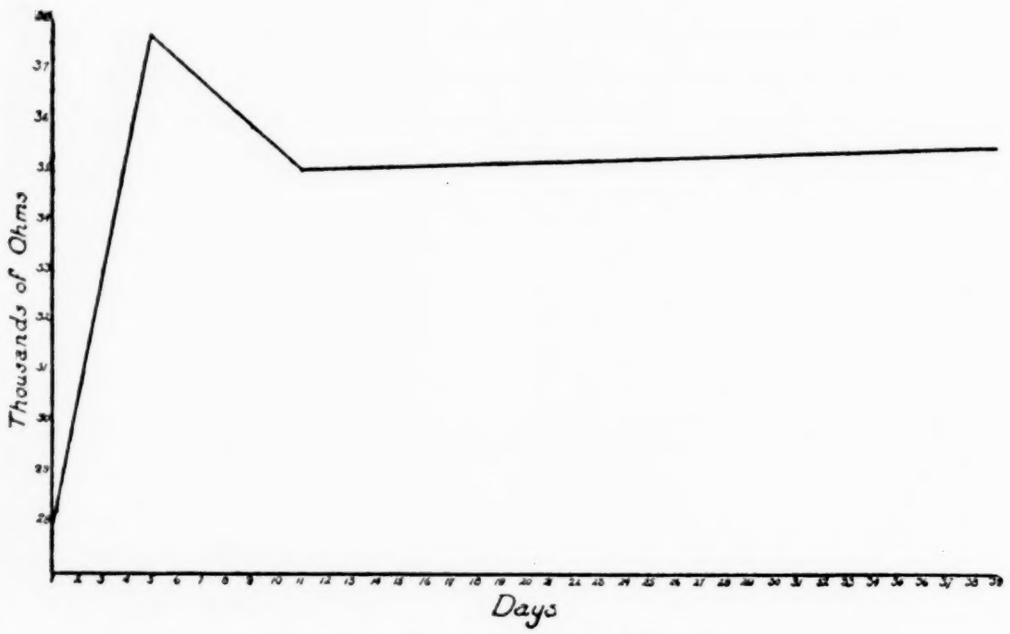

FIG. 5. Exp. II. Initial resistance.

ing signal is closely related to it. The general situation is more remotely related to the primary stimulus, and once the subject is habituated to it he tends to stay so.

EXPERIMENT III.—PHYSICAL EFFECTS IN THE ADAPTATION CURVE

A very prominent feature of the daily resistance curves shown in Fig. 1 is the great decrease during the fore period, when the subjects were subjected to no controlled stimulation. Such a falling off in the average resistanse curve is the regular finding. Occasionally a subject will exhibit a rising curve of resistance over a greater or less length of time, but his

tendency is so outweighed that the average seems always to fall. Wechsler (**28**) reported that no adaptation curve is found when electrodes are connected to the finger tips. Since he gives figures on but one subject it appears that he was mistaken. It is a common belief that the fall is caused by penetration of the electrolyte into the skin, perhaps under the force of the electric gradient. This seems rather doubtful in view of the nature of body resistance, but undoubtedly there are physical factors tending to depress the resistance value. In the paper (**5**) already cited the writer suggested that the physical factors were insufficient to account for the whole drop, since there is good reason to suppose that the subject is stimulated by uncontrolled elements of the situation, such as noises heard from other parts of the building, the strain of sitting in the same position, the subject's train of thought, and anticipation of the stimulus to come.

To test this hypothesis, two groups of subjects were used, four in each group, each subject having two sittings separated by one day. For one day's sitting for all subjects in both groups the usual current was allowed to flow for 11 minutes, readings being taken at regular intervals. No stimulation was used. For some subjects this sitting came the first day, and for some the second. For the other sitting of the first group the procedure was similar except that current was turned off after the first reading had been taken, turned on again for 30 seconds while a reading was taken at the fifth minute, and again at the eleventh. During the other sitting of the second group the subject removed his fingers from the electrolyte except when readings were being taken, at the same intervals as those for the first group. The solution, of course, tended to dry off the subject's fingers when they were withdrawn. From these data it is possible to compare change during current flow, with change when no current is flowing, both with and without immersion of the fingers. The initial resistance and the amount of decrease during the sitting under the conditions described are given in Table III. It is to be seen that a large decrease in resistance occurs whether the subject is connected to the instrument or not, and whether his

TABLE III

EXPERIMENT III

Change in Resistance Under Four Conditions

	With Current	Without Current	Fingers in Electrodes	Fingers Withdrawn
Initial Resistance	33,480	30,295	41,446	38,145
Final Resistance	25,370	21,260	27,540	31,253
Difference	8,110	9,035	13,906	6,892

fingers are immersed or not. It must be concluded that a large part of the change is caused by factors not connected with the technique of measurement. Since it is not possible for a subject's resistance always to be going down, it must be granted that the operating factors are psychological, *i.e.*, functions of the situation to which the subject is subjected.

If the drop is a response to stimulation one might expect to find habituation in this reaction also. But examination of the figures in Experiment I and in subsequent comparable experiments shows no consistent tendency in one direction or the other from day to day. Probably this should be expected on the grounds that the stimuli the subject may be supposed to respond to during the fore period are not the same from day to day, and habituation would have no chance to operate.

EXPERIMENT IV.—THE EFFECT OF DAILY REPETITION OF A LOUD SOUND USED AS A DISTRACTOR

In this series the same general procedure was used as in Experiment I, except that the situation was complicated by having the subject at the end of the fifth minute of the fore period begin to add prepared numbers, and continue until the end of the sitting. At the ready signal the subject was instructed to stop work on the problem on which he was working, put a mark, and start a new problem immediately. The subject therefore worked $1\frac{3}{4}$ minutes undisturbed and $1\frac{3}{4}$ minutes with noise or expectation of noise. Electrodes were connected to the left hand instead of to the right. A sheet of addition problems was attached to the writing arm of the subject's chair so that it could be turned right side up with

little effort when the signal was given. Subjects were instructed to work as fast as possible. As in Experiment I a ready signal was given 15 seconds before the noise.

For purposes of comparison with the group in Experiment I, the first day's sitting was conducted without the use of the problems. The group consisted of six men and four women.

Serious difficulty was introduced into this experiment, which was performed in the month of July, by the steady increase of the temperature. The noon reading on the first day was 80° and on the last day of the experiment 95°. It is known that the level of resistanse is lowered by increased temperature, and it is possible that the size of the reflex changes is also affected. This fact will, therefore, need to be considered in the interpretation of results.

Results as presented in Table IV. Fig. 6 presents graphically the course of the reactions to the ready signal and to the

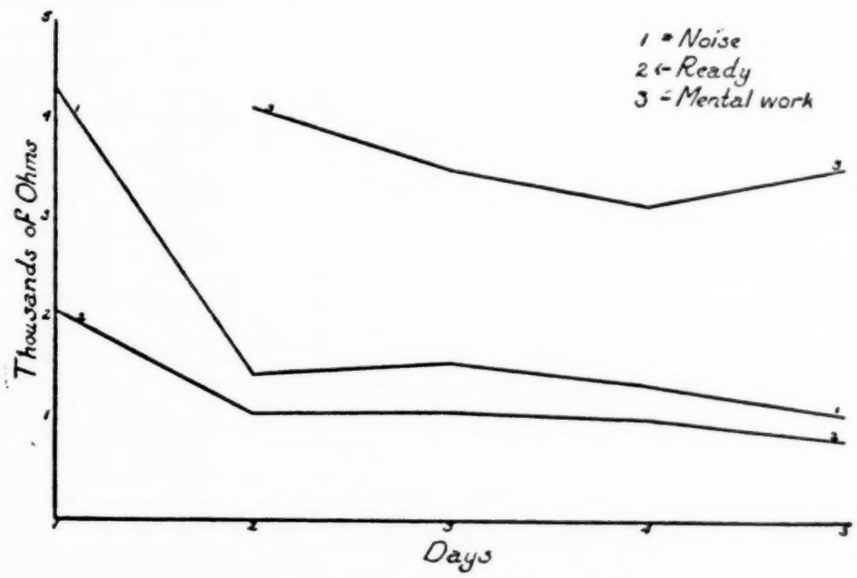

FIG. 6. Exp. III. Resistance drop.

noise. Although the absolute magnitude of these reflexes is about twice that of the reflexes in Experiment I, the general course of the habituation curve is the same as found when no mental work was used. There is negative acceleration and a tendency to be asymptotic to the time axis. The relatively greater flatness of the curve for the last three days is very

likely a temperature effect. It is concluded that habituation takes place in much the same manner whether noise is used singly or as a distractor. The difference in absolute magnitude of the responses under the two circumstances is taken to indicate only different levels of reactivity in the two groups.

Inspection of Table IV shows that the initial resistance does not show the trend it did in Experiment I; it goes neither up nor down any considerable amount. The steady increase in temperature during the week would lead one to expect such a result, since it would drive the resistanse in the opposite direction from the habituation effect.

Of special interest in this experiment is the reaction resulting from the mental work. It may be seen from Table IV

TABLE IV

EXPERIMENT IV

Average of Resistances During Successive Sittings

Time	1st Day	2d Day	3d Day	4th Day	5th Day
1	33,519	32,033	33,384	32,402	32,208
2	30,664	28,982	30,755	29,599	28,909
3	29,288	28,213	28,874	27,881	26,478
4	26,692	25,179	28,029	25,802	25,575
5	26,066	*24,525	*26,483	*24,883	*24,388
5.5	25,562	21,123	23,364	22,555	21,293
6	25,873	20,349	23,034	22,137	21,101
6.5	25,292	20,078	23,027	21,588	21,016
6.75 (Ready)	24,908	20,258	22,878	21,560	20,735
Min.	22,754	19,067	21,659	20,515	19,865
7	24,714	19,249	22,006	20,544	19,637
Min.	20,399	17,751	20,423	19,178	18,563
7.25	20,995	18,655	20,960	19,831	18,838
7.5	21,197	18,876	20,953	19,708	18,849
8	21,679	19,180	21,031	19,609	18,815
8.5	22,152	18,968	20,883	19,591	18,799

* Mental work began here.

that a sharp drop in resistance, comparable in magnitude to that following noise, occurred when mental work was begun. The change, however, was in some subjects of rather different character. In these there seemed to be no definite minimum followed by at least a slight recovery as was the rule in the reactions following the onset of the noise. There was, on the other hand, a continued, fairly rapid decrease. The readings

of minima are in these cases, therefore, a very uncertain magnitudes. During the $1\frac{3}{4}$ minutes the mental work continued before the sound began, it may be seen, there was a tendency for the drop to continue, a tendency not in evidence in reactions to noise. The third line in Fig. 6 is drawn to show the course of the reaction from one sitting to another. For the reason that the minimum is not reached in the first few seconds the total amount of change occurring between the beginning of work and the ready signal which preceded the noise was taken as but representative. It is to be seen that no change of reliable magnitude occurs through the four sittings.

It is desirable to examine the amount and quantity of work accomplished in relation to the resistance changes occurring under the two conditions. Amount of work accomplished is represented by the number of addition problems done (credit was given for problems partly done). Results appear in Table V. Both the number of problems attempted

TABLE V

EXPERIMENT IV

Problems Attempted and Problems Correct in Quiet and Noise

		2d Day	3d Day	4th Day	5th Day
Problems Attempted.....	Quiet	7.57	7.77	8.22	8.49
	Noise	6.92	7.21	7.18	7.64
Problems Correct.....	Quiet	7.18	8.02	7.72	7.48
	Noise	6.33	7.06	6.66	6.88

and the number right were less during the noise period than during quiet. To test whether the decrease is attributable to fatigue, a set of the same problems was given to a group of 13 subjects, who simply worked for two two-minute periods with no interval between. The results:

	First Period	Second Period
Av. No. attempted.....	8.8	8.5
Av. No. correct.....	7.6	7.3

Since the differences are smaller here it may be concluded that the noise probably had a distracting effect, reducing the

amount and quality of the subject's work. While noises of longer duration have usually been found to increase the output of work, their first effect has been shown to be interference. (Morgan (**21**),). The absolute discount of interference shows little or no indication of diminishing from day to day, but when expressed in percentage, evidently the more valid measure, it decreases from 12.3 percent to 7.5 percent, or about 40 percent, from the first to the last day, considering number of problems correct. As measured by number attempted the percentage changes only from 9 percent to 10 percent. Errors, of course, when expressed as percentage, show the same tendency as the number right. During the four days when the subjects performed the mental work their average resistance change in response to noise decreased approximately 25 percent. Indications are that their decrease is paralleled by decrease in the interference with mental work as measured by problems right, as far as the crudity of this latter measure might allow.

At first thought the implications of the Experiment IV are rather puzzling. At one time a large change in resistance accompanies mental work; at another an equally large change accompanies a decrease in the efficiency of mental work. The solution evidently is that in this experiment at least, the size of the resistance change is an indication of the force or exertion of the subjects' total reaction to the stimulus situation. During the noise greater exertion per unit of accomplishment is required than is necessary during quiet. Part of this additional activity is doubtless direct response to the noise; part may be increase in exertion in the mental work activity. This explanation also accords well with the continuous nature of the resistance drop during mental work, and the habituation to noise which occurs in the resistance reaction and probably in the efficiency of work. The question whether a decrease in resistance is favorable to efficiency in mental work would, therefore, be answered as follows for the present experiment: "The resistance change is a necessary part of the mental work reaction; it is also a part of other reactions which detract from the excellence of mental work."

Whether the above explanation will require revision or not the data of this experiment do show certain relations between the skin resistance changes and overt reactions.

Experiment V.—Habituation of the Resistance Reaction to Mental Work.

The absence of any marked decrease in the size of the resistance change during mental work in the quiet period on succeeding days in Experiment IV suggested the advisability of a further test. In Experiment V the subjects began work, on the same problems used before, after five minutes rest, and continued without interruption or further stimulation for two minutes. Data were obtained from two men and six women, five successive days. Readings were taken as before except that no attempt was made to record the reaction immediately after the onset of work, as the previous experiment showed there was no reaction peculiar to that circumstance. Temperature during the experiment was fairly constant. On the last day each subject was instructed to make a special effort to surpass his previous accomplishment in number of problems.

The data yielded by this experiment are exhibited in Table VI. Figure 3 shows the course of the average initial resistance

TABLE VI

Experiment V

Average of Resistances on Successive Days

Minutes		1st Day	2d Day	3d Day	4th Day	5th Day
1	Initial Resistance	34,975	35,309	37,468	38,064	40,565
5	Beginning of Mental Work	28,193	28,618	30,444	35,364	35,923
7	Final Resistance	21,321	24,059	26,141	28,256	25,074

during the series of sittings. As temperature was not an important variable here the conclusion of Experiment I, that initial resistance shows a change due to habituation, is confirmed. The increase in this series is smaller, however, being about 15 percent, perhaps because the level of resistance found on the first day was considerably higher.

The reaction during mental work, as measured by total change was even larger than in Experiment IV. A graph of its course is shown in Fig. 7. It is clear that no decrease in

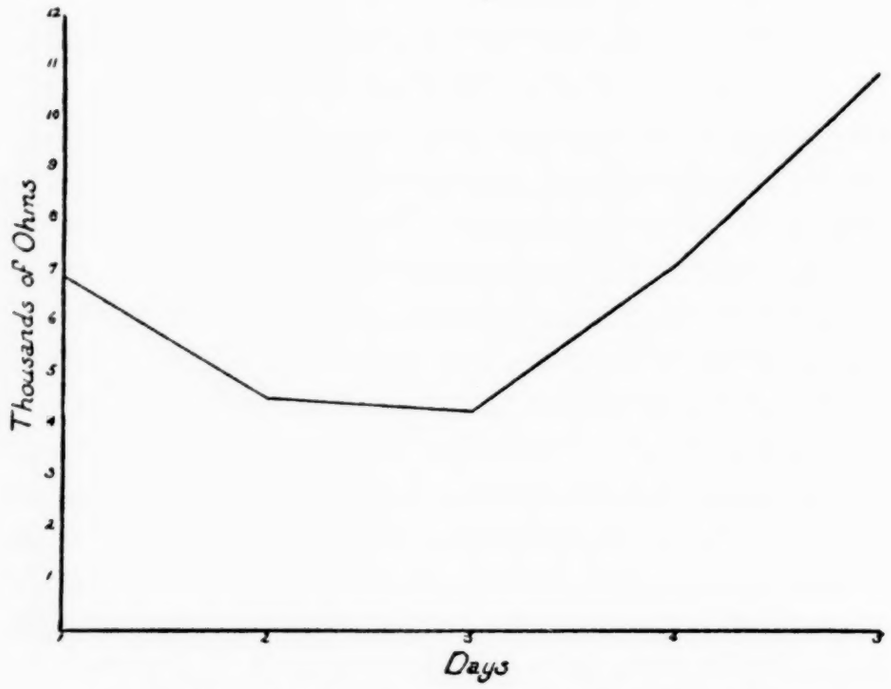

FIG. 7. Exp. V. Course of resistance drop during addition.

reaction upon repetition occurred. The definite increase in reaction on the fifth day can doubtless be attributed to the extra incentive to work given on that day. This experiment, therefore, confirms the indication of the preceding; there is no discernible decrease in the resistance change accompanying mental work when the performance is repeated.

This finding has considerable resemblance to that reported by Rounds, Shubert, and Poffenberger, (**22**) working with metabolism during mental work. They found the metabolic change at successive sittings remained practically constant. The number of problems solved increased, so that the expenditure of energy per problem decreased somewhat.

The output of work (number of problems attempted and number correctly solved) in the present experiment is represented in Table VII. In Experiment IV there is a tendency for the resistance change per problem to decrease. Leaving the fifth day out of consideration because of its special condition

TABLE VII

EXPERIMENT V

Problems Attempted and Problems Correct

	1st Day	2d Day	3d Day	4th Day	5th Day
No. attempted.....	11.0	10.4	11.4	11.0	12.4
No. correct.....	10.8	9.9	10.5	10.6	11.9

there is no observable trend in the change per problem in Experiment V.

On the final day, it may be seen both output of work and amount of resistance change increase. This increase in both is evidently caused by the extra motivation supplied on that day by telling subjects to try to surpass their previous accomplishments. This fact adds much weight to the explanation offered in the discussion of Experiment IV, namely that the resistance change is an indication of the degree of activity in the individual, whether directed toward an accomplishment or not.

EXPERIMENT VI.—COURSE OF RESISTANCE CHANGE WHEN NO STIMULUS IS USED

In view of the findings thus far reported on resistance change during a period of stimulation it is desirable to examine the course of resistance during a comparable period when no controlled stimulus is applied. Subjects underwent the usual procedure in the first part of the sitting. Five readings were taken at one-minute intervals; thirty seconds after the fifth reading a ready signal was given. Subjects were warned to begin with that this would be given, but that it signified the onset of no stimulus. A reading of the minimum resistance was taken and the regular series of readings at intervals of one minute was continued until a total of eleven had been obtained. Five subjects, all men, continued the experiment for the usual five consecutive daily sittings.

The average resistance at the critical times are set forth in Table VIII. As compared with other groups the initial resistance in this was extremely high on the first day. There is

TABLE VIII

EXPERIMENT VI

Average Resistances During Successive Sittings

Minutes		1st Day	2d Day	3d Day	4th Day	5th Day
1	Initial Resistance	29,700	30,592	27,674	28,959	28,092
5.5	Ready	24,288	22,860	18,810	19,473	23,321
	Minimum	23,031	21,949	18,413	19,282	22,747
6	Beginning Test Period	20,417	21,563	18,322	19,198	22,083
11	Final Resistance	17,946	19,413	16,305	17,423	21,352

no tendency for it to increase during the series. Since the subjects knew even before the first sitting that no stimuli were to be used, it is highly probable that the reaction which usually occurs before the sitting, in anticipation of it, failed to occur in this group to any extent.

The average magnitude of the reflex response to the ready signal, and the average daily decrease in resistance from the sixth minute-interval reading to the end of the sitting are plotted in Fig. 8. Response to the ready signal shows the

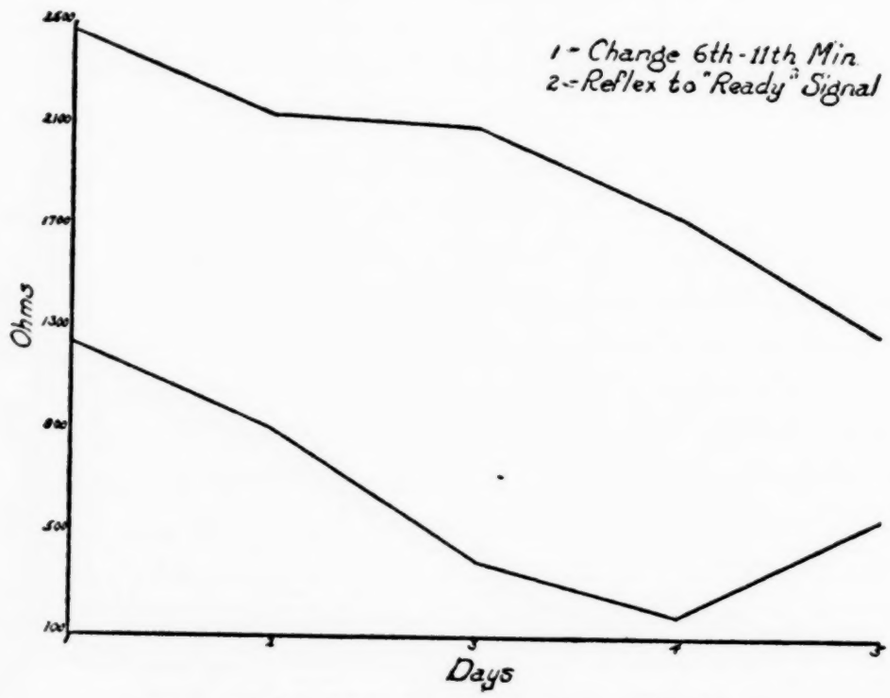

FIG. 8. Exp. VI. Reactions on succeeding days.

same trend as in Experiments I and III. The resistance drop during the 'critical period' shows a tendency to become

smaller on succeeding days. Its absolute magnitude on the first day is comparable to the magnitude of the response to noise in Experiment I, but little significance is to be attached to this similarity because of the individual differences involved. There is an important difference between the response to noise and the response in the present control situation: the former occurs all at once at the beginning of the period, while the latter at least when the average is considered, is a rather steady decrease throughout the period.

The fact that this decrease in resistance change takes place may be taken as indicating that psychological factors are involved; that there are responses to stimuli which tend to disappear with repetition. This experiment therefore, further confirms the conclusion of Experiment II, regarding the nature of the resistance curve. Whatever incidental stimuli, external or internal to the subject, produce this reaction, their effect resembles that of the noise stimulus in becoming diminished. It would not be expected that they would operate in addition to the noise stimulus in Experiment I, as the noise would be pretty sure to mask them almost entirely. They might conceivably be active during the mental work experiments, and possibly account for the relatively small decrease in the reaction during mental work which occurred on some days.

Experiment VII.—The Effect of Daily Repetitions of a Selection of Classical Music

Thus far certain stimuli have been shown to produce decreasing reactions when repeated, and certain others fairly constant. To uncover all the possibilities of the field some search was made for a stimulus which would produce an increasing reaction upon repetition. Since it seemed possible that "classical" music might have that effect an experiment similar to the preceding was devised, using as a stimulus a phonograph record of the 'Christe' from Beethoven's 'Missa Solemnis,' rendered by chorus, organ, and symphony orchestra. The record was selected as having great complexity of counterpoint, wide variations in loudness, solo passages by

different voice qualities, and a presumed ability to arouse a strong emotional response. Reproduction was secured by means of an electric pick-up, a Loftin-White amplifier and a dynamic loud-speaker. The turntable was located beside the experimenter, and the loud-speaker faced the subject's booth. The screening on the top of the booth allowed the sound to enter unimpeded.

Subjects in this experiment were six advanced students in the School of Music, one man, five women. Such subjects were chosen as being most likely to show some reaction to music. None of them had previously heard the selection so far as they could remember. They were fully instructed as to the procedure of the experiment before the first sitting, and were told simply to listen to the music when it was being played. Five readings at one-minute intervals were taken before stimulation was introduced. Thirty seconds later a ready signal was given and thirty seconds after that the record was started. It ran approximately four minutes. Minimum readings were taken after the ready signal and after the onset of the music. During the music readings were taken at half-minute intervals.

At the end of each sitting each subject was asked to give a written description of his reaction answering the following three questions:

1. How well did you like the piece?
2. How exciting was it?
3. What parts, if any, particularly attracted your notice?

After the first day the subjects were asked to compare their reactions with their previous ones with respect to the first two inquiries.

The usual averages of resistance readings are shown in Table IX. Initial resistance here shows the expected upward trend, though in rather irregular fashion. At the onset of the music there was usually an immediate reaction resembling the reflex to noise, but much smaller. After this during the music there was a steady downward drift of average resistance; caused partly by rapid changes in individuals, (reflexes),

TABLE IX
EXPERIMENT VII
Average Resistances on Successive Days

Time	1st Day	2d Day	3d Day	4th Day	5th Day
1	41,520	40,798	49,608	47,533	46,863
2	35,060	34,780	44,508	40,450	42,663
3	32,063	31,782	40,538	38,205	40,348
4	31,670	30,187	37,065	35,388	38,353
5	30,090	28,497	34,997	34,173	36,495
5.5 (Ready)	30,350	28,332	34,133	33,513	35,557
Min.	29,573	27,300	33,778	33,048	35,116
6 (Music)	28,312	27,210	33,332	32,780	33,978
Min.	27,585	26,700	33,068	32,445	33,485
6.5	27,493	26,703	32,885	32,327	33,308
7	27,267	26,645	32,665	32,030	33,158
7.5	27,332	26,420	32,120	31,778	32,540
8	27,072	26,592	31,693	31,828	31,937
8.5	26,778	26,567	31,328	31,542	31,385
9	26,773	26,710	31,202	31,555	31,168
9.5	26,710	26,627	31,142	31,567	31,060
10	26,683	26,605	31,083	31,532	30,988

and partly by slow ones. (It would be impossible to draw a distinct line between the two.) Hence there are two measures of reaction which it was deemed wise to treat separately; first, the initial reflex, and second, change during the music, exclusive of the initial reflex. These two, with the reflex to the ready signal, computed as before, are shown in their temporal course in Fig. 9. Though less regular than in previous experiments, the reflex to the ready signal shows the usual downward trend. The reflex to the onset of music also decreases, rather irregularly, being on the average about 40 percent less after the first day. Evidently the reaction resembles the reaction to noise. As it occurs following upon the opening notes or perhaps upon the preliminary needle scratch, this result is not surprising.

The reaction to the music exclusive of the initial reflex, on the other hand, shows a definite increase, though irregularly, being about three times as great on the last day as on the first. The reaction on the last day is of the same general magnitude as the reaction to noise on the first day in Experiment I though of course different in not being sudden in occurrence. The general form of the curve would be the same if changes were expressed as percentages of total resistance. This trend

is particularly significant in view of the results of Experiment IV, which showed that without controlled stimulation the reaction during a 'test' period tended to decrease. If the incidental stimuli discussed in the last experiment are active in this case (which is somewhat doubtful) the true measure of the course of the music reaction would show an even greater increase.

Concerning the resistance change during a single sitting it is not possible to analyze completely with the present data

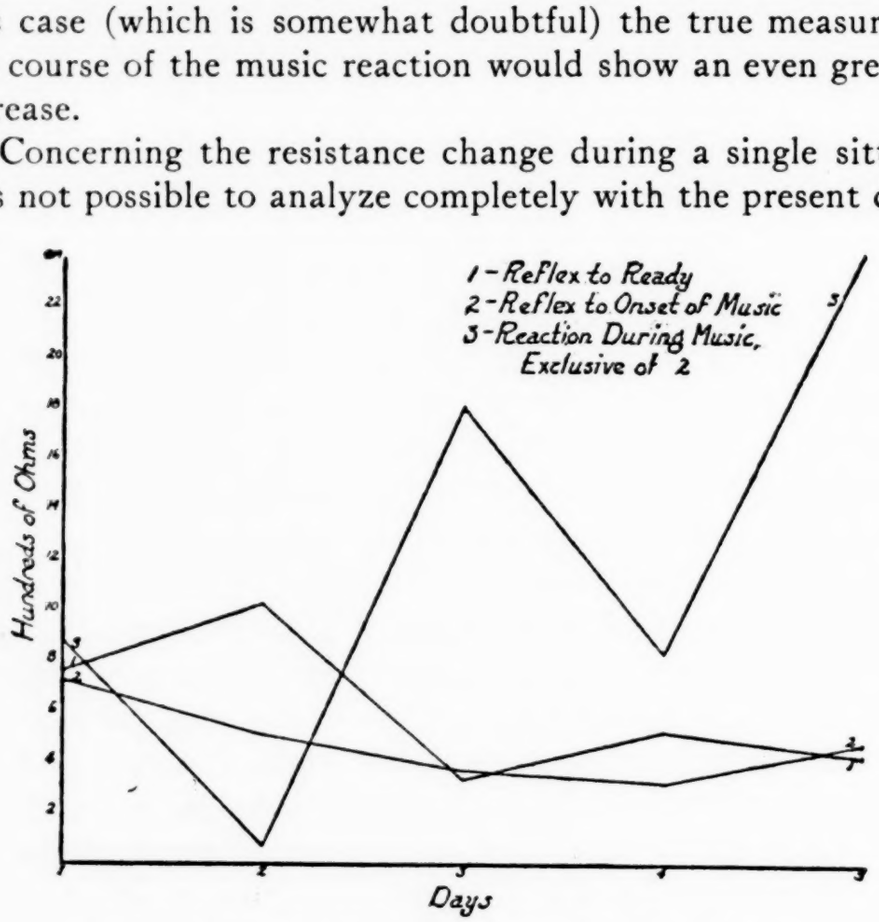

FIG. 9. Exp. VII. Course of reactions on succeeding days.

what features of the music acted as stimuli for the reactions which occurred, but it is possible to note the stimuli for certain rapid reactions which frequently took place. Reflexes, as would be expected, sometimes occurred at a sudden crescendo, but were not large enough or general enough, to produce an irregularity in the line representing the average resistance. Subjects also often gave a reflex to a certain sustained soprano note, the peculiar quality of which was frequently remarked upon in the subjects' reports. The average resistance shows a steady decrease beginning at the end of the first minute and lasting about a minute and a half. The writer could not detect any characteristic of the music peculiar to this period.

For purposes of comparison, the subjects' reports were analyzed on the basis of the three questions asked. A rank order was constructed for each subject's replies to the first two questions, the sitting for which the report of greatest liking or excitement was given being ranked five, and that with the least, one. Ties were given their average for a ranking, as usual. One subject reported no excitement on any occasion. All others were able to give degrees of liking and excitement. The average ranking of each sitting by all subjects was then computed for each of the two characteristics. To study replies to the third question, the total number of items mentioned each day was counted. The results of these three calculations are shown in Table X, and the first two are graphed in Fig. 10.

TABLE X

AVERAGE RANK GIVEN EACH DAY'S HEARING AND TOTAL NUMBER OF ITEMS REPORTED

	1st Day	2d Day	3d Day	4th Day	5th Day
Av. Rank—Liking.....	1.25	2.50	2.75	3.75	4.66
Av. Rank—Excitement.....	1.92	2.42	3.16	3.25	4.16
Total No. Items.....	18	12	16	13	14

Judgments of liking and excitement show a definite increase, resembling the increase in the resistance change.

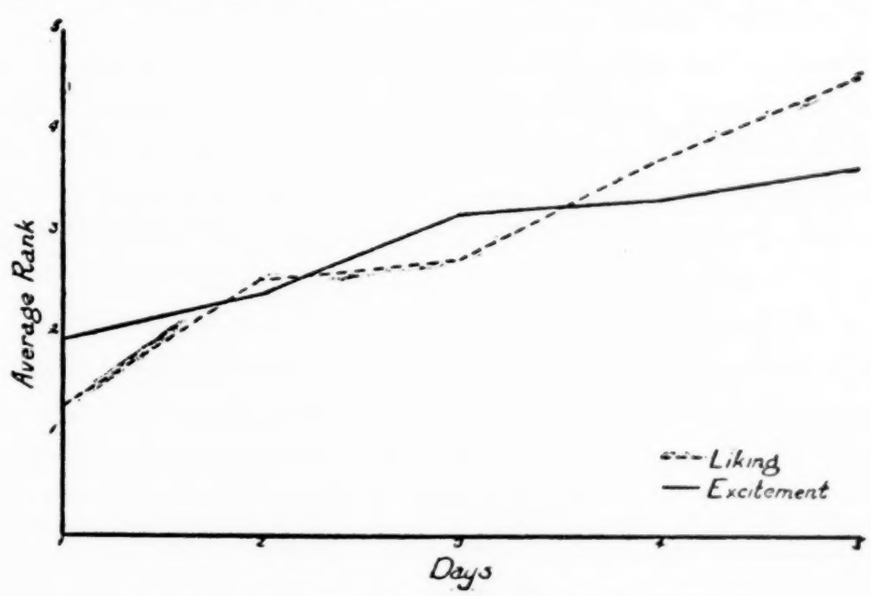

FIG. 10. Exp. VII. Average rank of daily repetitions.

The excitement curve even shows some irregularities corresponding to those of the resistance change, though probably little weight is to be attached to the fact. The number of items reported shows no certain trend, but the interpretation is not clear, as it appeared some subjects understood they were to report only things not previously noticed. The increase in the subjects' reports of liking would be expected from the results of the experiment by Gilliland and Moore (**14**).

In summary, Experiment VII yields these additional facts: (1) there is a reflex to the onset of music which decreases upon daily repetition of the stimulus; (2) there is a reaction during the course of this selection which increases from day to day; (3) this increase is accompanied by an increase in liking and excitement as reported by the subjects.

General Discussion

The series of experiments here reported demonstrates that it is not possible to make any simple general statement regarding the wearing out of this emotional reaction upon the repetition of the stimulus. With certain stimuli the reaction decreases from day to day, with others it increases, and with still others it remains essentially unchanged. It is possible that the reaction to the musical selection would eventually decrease if the experiment were prolonged; so might the reaction to mental work, though with less likelihood.

The experiments show that the reactions in question may be rather easily modified by repetition of stimuli. Many less repetitions are required than is the case with such reflexes as the knee-jerk, wink reflex, and nystagmus which have been investigated by Dodge (**9**) ,(**10**). There is of course the further difference that the direction of modification is not always the same.[1]

The different speed and duration of the reactions to different types of stimuli suggest that possibly there are different

[1] In spite of these differences and others, the writer sees no reason why the galvanic reaction should not continue to be called a reflex. There is no exactly defined concept of 'reflex' so far as he is aware. The general sense of the word seems to be 'non-voluntary' and the galvanic reaction is undoubtedly that. It resembles other reactions called reflexes, and has through fifty years been called by that name.

mechanisms of response in operation. Richter (**23**) suggested that there are two relatively independent mechanisms in the skin, the sweat gland, and the epithelial cell. On the tips of the fingers the sweat gland mechanism would be expected to predominate, but the other may be present also, and reactions may be a combination of the two in different proportions. The rapid reactions, as to noise, would be predominantly of the sweat glands, and the longer reactions, as to music and mental work, would be predominantly epithelial. It might even be possible that it is the sweat gland mechanism which exhibits habituation, and the epithelial which does not. The possibility requires further inquiry.

In discussing the results of Experiment IV, it was suggested that the reaction here measured is large when the total activity of the individual is large, whatever the relation of that activity to the stimulus. This theory also leaves room for further questions concerning the experimental results: (1) What is the general nature of the stimuli which produce this large activity? and (2) What stimulus characteristics determine an increasing, decreasing, or constant reaction? Regarding the first, very general question, the present series yields little information. Concerning the second, a number of hypotheses would harmonize with the present results. Karsten (**18**) in discussing an experiment on the adaptation of hedonic tone, remarks that decrease in the effect is not a direct function of repetition alone. A similar statement would apply to these results.

For the present data any of the following hypotheses may be correct:

1. Stimuli which produce a pleasant reaction bring about greater reactions upon repetition; those which produce an unpleasant reaction, cause decreased responses upon repetition, and indifferent stimuli continue to have about the same reaction. This statement is of course, the familiar law of effect in one form. The stimuli in this experiment do have the characteristics required by this hypothesis: the noise, the ready signal, the incidental stimuli and the onset of music may very well have an unpleasant character for the subject, the

music a pleasant character, and the mental work an indifferent. Yet the hypothesis would be subject to the usual criticisms of the law of effect, particularly as to the relation between 'pleasantness' subjectively determined, and the reaction. It still fails to reach the final problem, of distinguishing between stimulus characteristics.

2. Stimuli composed of a greater number of elements produce constant or increasing reactions. Certainly the stimuli with decreasing reactions are relatively simple. The mental work is more complex, but probably not so complex but that reaction could be made to all aspects of it on the first day. The music, as may be seen from the subjects' reports, had aspects not observed until some repetitions had been given. Taken all together, these aspects might be considered to constitute a different or stronger stimulus on each occasion. The perception of the new aspects, or their relationships may be the activity which is indicated as diminishing, increasing, or remaining constant. This principle does have the merit of describing fundamental properties of the stimuli.

3. The reactions examined are found in different stages of development. At first repetition increases, then decreases response. This hypothesis would have some difficulty with the reaction to mental work, as it remained essentially unchanged. It also goes much further beyond the immediate evidence than the two previous suggestions in assuming widely different past histories for the stimuli concerned. As a matter of fact, all the stimuli are, strictly, new to the subject, and if this third hypothesis were to be accepted it would be necessary to suppose different amounts of transfer in the various reactions.

Further experimental work is necessary before a choice can be made from these hypotheses, or possibly another constructed. Some of this research is now under way.

Summary

A series of seven experiments was undertaken to study the habituation of the galvanic reflex and body resistance to repetitions of various types of stimuli. Groups of subjects

in most of the experiments were subjected to five daily repetitions of the stimuli.

The following stimuli showed a decreasing effect during the five days: a noise while the subject was engaged in work or while he was 'resting,' a ready signal which was to be followed by stimulation, and the onset of music.

During mental work there occurred a slower reaction lasting throughout the period of work. This remained practically constant through the five-day period.

Reaction to a piece of classical music, aside from the reaction at its onset, increased over a period of five days.

Instruction to a group to increase their output of mental work caused a marked increase in the reaction accompanying the work.

Initial resistance regularly showed a pronounced increase over a period of five days. It is suggested that a reaction occurred before each sitting began, and that this diminishes in size with successive repetitions.

A control group showed a drop in resistance during a period comparable to the period of stimulation in other groups. This drop decreased in five successive sittings. The decrease is interpreted as habituation to incidental stimuli.

A group of subjects was re-tested six days after the end of the habituation series using noise, and again thirty-three days after the end of the original series. The reactions to the ready signal and to the noise had partially recovered after six days, and almost entirely after thirty-three. The initial resistance tended to remain at the same level as on the last day of the habituation series.

A major part of the usual "adaptation curve of resistance" during a sitting was found during a period in which subjects were in no way connected to the instrument. It is concluded that a substantial part of this 'adaptation curve' is not an artifact of measurement, but a response to the experimental situation.

It is concluded that the data imply a relationship between the resistance change and degree of activity, either directed or undirected in the subject.

The possibility of the involvement of different reacting mechanisms in the various responses is discussed.

It is concluded that decrease of response is not a simple function of repetition of stimuli. Several hypotheses regarding determining factors for increasing and decreasing responses are discussed.

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THE INFLUENCE OF PUNISHMENT ON LEARNING *

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Explanatory Statement and Summary

The material in this paper does not fall into the mold of one discrete study. It consists rather of a series of experiments each of which bears on the rôle of punishment. In the series the results of one study set the problem for the next. It seems more convenient to present the material in that order, and to try to add some coherence by means of an explanatory statement.

Some recent papers [1] on learning indicate that, in certain conditions, punishment has little or no effect. A subject when told that he is wrong, is more likely to persist in his original choice than he would be if he were told nothing. This holds true when (*a*) the influence of punishment is tested 24 hours after it is applied, (*b*) the punishment is conveyed by verbal means, and (*c*) the learning task is of a highly verbal nature.

It seemed interesting to check these results against data gathered under different conditions. I had already to hand a body of data totalling some 20,000 responses which were obtained in another problem and for a different purpose. These data were secured under the following conditions: (*a*) the influence of the punishment was tested within a few seconds

* I wish to acknowledge important aid at various points in this study. All the high school subjects were secured through the courtesy of Mr. John Schwatka, Principal of School 70. The college students in experiment IV were tested by Mr. Arthur Lichtenstein and Dr. J. A. Baer.

[1] Thorndike, E. L., *Human Learning*, New York, Century, 1931. Thorndike, E. L., *Fundamentals of Learning*, New York, Bur. of Pub., Teachers College, Columbia University, 1932. Thorndike, E. L., The influence of irrelevant rewards, *J. Educ. Psychol.*, 1933, **24**, 1–15. Lorge, I., The efficacy of intensified reward and of intensified punishment, J. Exper. Psychol., 1933, **16**, 177–207. Trowbridge, M. H., and Cason, H., An experimental study of Thorndike's theory of learning, *J. General Psychol.*, 1932, **7**, 245–260.

after its application, (*b*) the punishment was conveyed by mechanical means, making possible a control over the quality of the signal and the immediacy of its application not available to verbal signals, and (*c*) the learning task was of a motor nature (attempting to hit an unseen target).

It was found that under these somewhat different circumstances punishment still had a stamping-in effect.

It seemed that this apparently anomalous influence of punishment might be due to a possible stamping-in effect of the physical medium by which both the punishing information and the rewarding information were conveyed. That is, both punishment and reward must be conveyed by some physical medium. This medium may in itself have a stamping-in effect. Consequently any stamping-out tendency of punishment must contend with the stamping-in influence of the medium. Conversely any stamping-in influence of reward would be enhanced by the influence of the medium. For this reason any observed influence of punishment or reward ought to be corrected for the constant influence of the conveying medium.

In a new experiment designed to check such a possibility it was observed (1) that this physical medium, when divorced of all connotations of success or failure, did have a stamping-in influence; (2) that the stamping-in influence of the *medium plus punishment* might be regarded as *less* [2] than that of the medium alone; and (3) that the influence of the *medium plus reward* was definitely *greater* than that of the medium alone.

The experiment was then modified so that all information was conveyed by means of simple verbal statements rather than by means of mechanical signals. Under these conditions it was found (1) that the medium itself (a word carrying no information as to success or failure) still had a stamping-in effect but (2) that when this medium was used to convey punishment, the total effect was of a stamping-out nature. Although the incidence of some informationless remark after

[2] The difference was not found to be significant in the only data susceptible to the determination of significance. It was consistent, however, in three distinct groups.

a response caused the subject to repeat that response *more* often than would be expected on the basis of pure chance, the statement of the word 'Wrong' caused him to repeat the response *less* often than would be expected on the basis of chance. That is to say the influence of verbally conveyed punishment was strong enough to completely counteract the stamping-in effect of the medium and to add a balance in the stamping-out direction.

Since the other studies found that, with the opportunity for the next response deferred 24 hours, a statement of wrong had a gross stamping-in effect, it is suggested that the difference in the results may be due to the difference in the length of time elapsing between the application of the effect and the next opportunity for an act.

In conclusion it is suggested that (1) the influence of punishment varies with the medium by which it is conveyed and with the time elapsing between the application of the punishment and the next opportunity to repeat the act; and (2) that, in the type of learning reported here, the stamping-in effect of punishment is due to the medium by which it is conveyed, the punishment *qua* punishment tending to *reduce* the stamping-in effect of the medium.

Experiment I

As indicated above, the data for the first experiment had been collected in an experiment conducted for another purpose altogether.[3]

The subject attempted to hit, with a stylus, a particular spot on a horizontal square plate which he could not see. The plate was marked off into numbered squares, and the number of the square which the subject hit was recorded after each attempt. The procedure was as follows: Each subject was told that for the first series of hits no information as to the success of each hit would be given. He was to see how many times he could hit the right square by 'pure guess.' He made 20 hits in this fashion. These will be spoken of as the 'no information' hits and the series as the 'no information' series.

Upon the conclusion of the 'no information' series, one of the 7 different 'information' series was given. In this second series the subject was told (in the case of some subjects) that a light would flash immediately after each correct hit and that

[3] Stephens, J. M., *The Effect of Different Stimuli upon Preceding Bonds; an Examination of the Law of Effect*, New York, Bur. of Pub., Teachers College, Columbia University, 1931.

nothing would happen after an incorrect hit. The subject then made 51 hits and after each hit called aloud 'right' or 'wrong' according as the light had flashed or failed to flash.

A third series of 51 hits followed this second one in which an incorrect hit was indicated by (say) a shock in the hand and a correct hit by the absence of the shock.

In all, 7 such 'information' series of 51 hits each were used with each subject. With some subjects the 'light flashing' series came immediately after the 'no information' hits, and with others 'the shock in the hand' series occupied this serial position. Each of the 7 different 'information' series occupied, with one subject or another, each of the serial positions. That is to say, each one came first with some subjects, second with other subjects, third with others and so forth.

In this discussion, however, no attention will be paid to the different methods of signalling. Only the report of subject will be considered. All the hits which the subject judged to be right will be called 'right' hits and all those which he judged to be wrong will be called 'wrong' hits.

This provides for each subject: (1) a series of 20 'no information' hits, (2) 7 series of 51 'information' hits each. Each of the hits except the last one in each series could have been repeated. I give below a portion of a 'no information' series and a portion of an 'information' series to illustrate which hits would be counted as repetitions:

No Information Series		Information Series		
Serial Number of Attempt	Number of Square Hit	Serial Number of Attempt	Number of Square Hit	Subject's Interpretation of Information
1	66	1	28	wrong
2	72	2	38	wrong
3	72*	3	33	right
4	53	4	43	wrong
5	78	5	89	right
6	72	6	89*	right
7	37	7	99	wrong
8	37*	8	76	wrong
9	37*	9	73	right
10	17	10	73*	right
11	14	11	68	wrong
12	31	12	73	wrong
etc.		13	73*	wrong
to 20–		14	92	right
		15	27	wrong
		etc.		
		to 51		

In these illustrations the asterisks indicate the repetitions. It will be noted that only successive repetitions are counted

as such. For instance the 6th attempt in the 'no information' series and the 12th in the 'information' series are not considered as repetitions. It will also be noticed that in the 'information' series more than one square could be 'right.'

We are trying to determine whether repetitions are more likely to follow 'right,' 'wrong,' or 'no information' hits, and we are especially interested in the comparison of the 'wrong' and 'no information' hits. The relevant comparisons are given in the following table:

	Possible Number of Repetitions	Actual Number of Repetitions	Percent of Repetitions
'Right' Hits	10250	4368	42.60 ± 0.33
'Wrong' Hits	9282	1693	18.24 ± 0.27
'No Information' Hits	1064	142	13.35 ± 0.70
Difference between 'Wrong' and 'No Information' Hits			4.89 ± 0.75

We see that the hits followed by a signal of 'right' are repeated much more frequently (42.6 percent) than those followed by no information (13.35 percent). Those responses which were followed by a signal of 'wrong' were *also* repeated slightly *more* often (18.24 percent) than those followed by no information (13.35 percent). That is, it seems as if telling an individual he had hit the wrong place did not prevent him from returning to the same place on the next hit, but had a slight tendency to cause him to return.

Before this conclusion can be definitely accepted several points must be checked. In the case of each subject the 20 'no information' hits preceded the 350 hits for which information was given. It is quite probable that the tendency to vary the responses might decrease during the course of the series. In this case the greater repetition of 'wrong' hits may be due, not to the fact that the subjects were told they were wrong, but to the fact that they came later in the series.

To secure a partial check on this possibility I reworked the data using for each subject only the 20 'no information' hits and the first 20 hits for which information was given. (It must be remembered that this information was given by differ-

ent means for different subjects.) The results of the check are given in the following table:

	Possible Number of Repetitions	Actual Number of Repetitions	Percent of Repetitions
'Right' Hits	462	160	34.63
'Wrong' Hits	657	117	17.81
'No Information' Hits	1064	142	13.35

The relative frequency of repetitions of 'wrong' hits in the first 20 'informed' hits is practically the same as the frequency during the entire series.

A further check on this possible tendency to greater repetition as the series progressed, may be secured by dividing the 350 'informed' hits of each subject into the 7 sets of 50 each and determining (*a*) the percent of 'wrong' responses repeated and (*b*) the percent of all responses repeated. Thus if we combine the first 50 'informed' hits of all the subjects together we find that 19.88 percent of the wrong responses were repeated. This information for the first 50 'informed' hits and for each succeeding set of 50 'informed' hits is given below.

SETS OF 50 'INFORMED' HITS

	0–50	51–100	101–150	151–200	201–250	251–300	301–350
Percent of 'wrong' hits repeated	19.88	17.69	16.55	17.74	19.15	19.68	16.98
Percent of all hits repeated	30.70	30.09	30.49	29.60	32.45	32.95	31.04

The analysis shows no increased tendency to repetition (irrespective of punishment or reward) during the latter part of the series.

This possible tendency was again checked by calculating the number of repetitions at each serial position of the 20 'no information' hits. This is obtained by determining how many of the 56 subjects repeated hit no. 1, hit no. 2, and so forth. Thus in the illustration given earlier, hits no. 2, 8, and 9 were repeated.

These frequencies for all subjects combined are given in the following table:

NUMBER OF SUBJECTS WHO REPEATED EACH OF THE 'NO INFORMATION' HITS

Serial Position of the Hits...	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Number of Subjects Repeating.....	7	8	9	11	7	7	8	6	12	5	10	7	10	7	8	7	10	4	13

There seems to be no tendency toward greater repetition at the latter part of the series.

Any data which can be gathered from this experiment confirm Thorndike's data in indicating that in such types of experiment, informing a subject that he is wrong in any given response, does not decrease the likelihood of that response's being repeated. The subject is, if anything, more likely to repeat the response than he would be if he had been told nothing.

We come now to the problem of discussing the meaning of such apparently anomalous results. Thorndike, in the *Fundamentals of Learning*, page 311, attaches such importance to them as to invoke the congruity theory to explain them.[4] It is quite possible however that some constant factor is at work to mask the effect of punishment and accentuate that of reward. Rewarding an individual or punishing him both involve attending to him. This thing of receiving attention may have a stamping-in influence sufficient to overcome any contrary tendency of punishment. It may be more satisfying to receive attention of a disapproving nature than to be ignored.

For this reason it seemed important to compare the influences of punishment and of reward, not to what would have happened if no comment or signal had been made, but to what would have happened had a neutral comment or signal been made. Or, to put it in other words, it seemed wise to control the influence of the physical medium which conveys the rewarding or punishing information. This is what Cason

[4] This point has been developed at length elsewhere. See Stephens, J. M., A change in the interpretation of the law of effect, *Brit. J. Psychol.*, 1934, **24**, 266–275.

tried to do by the use of nonsense syllables. He found that the nonsense syllables disturbed the subjects.

Experiment II

In experiment II I tried to avoid the disturbing feature of a purely irrational or ridiculous comment or signal by using a signal which was understandable but carried no information as to success or failure. In this experiment the apparatus was very similar to that in Experiment I except that the only signals were the letters R and W hidden respectively by translucent paper. When no light was glowing the subjects looked at two plain pieces of paper behind two glass covers. When one light flashed the letter R appeared on one paper, and when the other light flashed the letter W appeared on the other paper.

The following directions were read by the subject before the beginning of the experiment:

"In this experiment you are to try to hit a certain part of a target. The target lies flat on a table and you try to hit it with a large pencil. You will not be able to see the target on account of a piece of cardboard between you and it.

"Each time you hear a 'click' you are to make one hit at the target and then quickly replace your hand at the resting position.

"In front of you, you will find two windows. After SOME hits a letter may appear in one of these windows. If R appears it means that you have hit the *right* place. If W appears it means that you have hit a *wrong* place. But after most hits no light will flash. In that case you won't know whether you are right or wrong. Remember that when no light flashes it does NOT mean that you are wrong. It just means that you are not being told this time.

"Sometimes there may be an accidental short circuit in the apparatus and then both lights may flash. In that case, you won't know whether you are right or wrong.

"At times the target will be shifted slightly. Then you will have to start all over again.

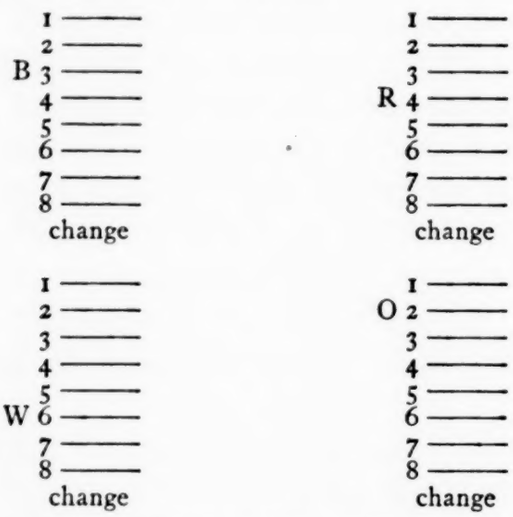
"Whenever a light does flash be sure to tell the experimenter. Just say 'right,' or 'wrong.' If it should happen that both flash, say 'both.' "

Procedure

The subject sat facing a beaverboard screen on the other side of which was the table. The plate or target lay flat on this table. The two signal lights were fastened to the screen and faced the subject. The subject placed his hand through an aperture in the screen. A funnel shaped cloth fastened around the aperture was drawn snugly around his arm. After the subject was seated and the cloth adjusted on his arm, he held the stylus while the experimenter guided it around the perimeter of the plate. The experimenter then demonstrated the lights, explaining the meaning of each of the three flashes. A click was then given and the subject made a hit at the target and

returned to the resting position. After this first hit some subjects had to be told not to hit so hard, while others had to be told to return to the resting place. The experimenter recorded the number of the square that the stylus had struck, and sounded the click again.

The subsequent procedure may best be discussed by reference to the accompanying portion of a record sheet, showing 4 series.



After the first and second hits in the first series the subject got no signal. Just before the third hit the experimenter depressed *both* keys thereby connecting both lights with the plate and so arranging that when the stylus struck the plate (no matter where) both lights would flash. During the remaining five hits of the series no signals were given. After the eighth hit the experimenter announced 'I am moving the target. You will have to start all over again.' For the first five hits of the next series no signal was given but just before the sixth hit the 'W' key was depressed, connecting the 'wrong' light with the target so that as soon as the stylus struck the target anywhere the signal 'W' would flash. At the end of this series a change in the position of the target was announced.

The other series were similar in nature, the signals coming somewhere between the second and the sixth hits. Thus in the third series of the illustration a signal of 'R' would be flashed on the fourth hit, no matter which square was struck. In the fourth series no signal was given, but the second hit was marked off as a control.

Each subject was given twelve such series making possible three records of each type of signal and three controls. The succession of different signals was quite at random, and varied from subject to subject.

The subjects for this experiment consisted of twelve students in the College for Teachers and nineteen high school students.

Results

As in the previous experiment the data were examined to see the relative extent to which responses followed by the respective signals were repeated. This information for each type of signal respectively is given in the following table:

	No. of Subjects	Right			Both			Wrong			Control		
		Same	Diff.	% S	Same	Diff.	% S	Same	Diff.	% S	Same	Diff.	% S
College students	12	19	18	51	10	24	29	7	29	19	1	35	3
High sch. students.....	19	20	36	36	35	22	62	17	40	30	10	48	17
Total.....	31	39	54	42	45	46	49	24	69	26	11	83	12

Under the heading 'Same' in the table is given the number of hits which were followed by hits to the same squares. Suppose, for instance, that a subject has hit square No. 23 and the signal R has flashed. If, now, the next hit is to square 23 that hit would be tabulated under 'Same.' If it hit any square other than 23 it would be tabulated under 'Different.' The next column gives the percent of all the hits producing the right signal which were followed by hits to the same square.

From the table several interesting facts arise:

First, in every case we find all three types of signals tend to cause a repetition of the responses they follow. If no signal is given we find that from 3 percent (in one group) to 17 percent (in the other group) of the hits are repeated at the next opportunity. All the hits followed by signals are repeated more often than that.

Second, in both cases we find that the hits followed by the meaningless flash of both lights are repeated more often than a flash which means 'wrong.'

Third, the two groups show marked disagreement as to the relative influences of 'both' and 'right.' In the case of the High School people the signal of 'both' is more likely to induce a repetition.

Discussion of Results

In so far as they are consistent the results indicate (1) that telling a subject he is wrong tended to make him repeat rather than to avoid the response in question; and (2) that a flash of both lights (which is supposed to mean exactly the same as no signal at all) is more likely to cause such a repetition than the signal of 'wrong.'

This latter was the question we set out to answer. We were endeavoring to see if the stamping-in influence of a signal of 'wrong' was due to the *concept* of wrong or to the fact that something happens after the response. The flash of both lights is such a neutral something. It gives no information as to right or wrong but it provides a happening of some sort. Such a happening has a very definite stamping-in influence.

It was hoped that a more refined analysis would be possible. Our thesis would imply that the 'something happening' is a *sine qua non* of any effect. This 'something' happening has in itself a stamping-in effect. Now if we find that a 'right' signal has more of a stamping-in influence than a neutral signal, and a 'wrong' signal less of a stamping-in influence than a neutral signal, we would still hold the concept of punishment to be the psychological opposite of reward.

Such a finer analysis did not seem advisable on account of the disagreement between the results of two groups. It was decided that such a disagreement might represent a real difference between the college and the high school groups, or it might be due to the very definite errors of measurement. It will be seen that the use of the squares to determine whether or not the response was repeated, was a very crude device. If the one response had hit the right edge of square 28 and the next one the left edge of square 29, the two responses would be considered different, though they may have been only 0.1 or 0.2 inches apart. On the other hand if the two responses had hit at opposite diagonals of the same square (1.4 inches apart) they would have been considered the same. To be sure, on the average, a pair of responses hitting the same square would have been closer together than a pair hitting

different squares. There was however a large chance of error. In addition, this technique took no account of the extent of the deviations beyond the fact that there was a difference. This, of course, provided more opportunity for error.

Experiment III

In order to determine the influence of this error and to, at the same time, remedy it, a new measuring device was used. In this experiment the plate was removed and for it there was substituted a tray made to hold sheets of ordinary mimeograph paper ($8\frac{1}{2}$ x 11). One end of the tray was hinged to a large board and a spring inserted so that the free end of the tray normally remained a slight distance above the board. In the free end of the tray were inserted two set-screws so arranged that each would dip into a mercury well whenever the tray was tapped from above. In this way a circuit could be completed when the tray was tapped with whatever instrument.

In this case the subject tapped with a large black marking pencil. This left a mark on the paper wherever it hit.

The procedure was as follows: The experimenter held in his hand 24 sheets of mimeographed paper so arranged that every other sheet was a 'test' paper and the others were 'practice' papers. On each practice sheet was written the number of practice hits which were to be given (varying as in the previous experiment from 2 to 6) and the nature of the succeeding test (*e.g.* whether Right, Wrong, Both or Control). On each test sheet was written the letters R or W or B or O.

The experimenter placed a *practice* sheet in the tray and gave the signal for a hit and repeated this signal as often as was indicated on the sheet (from 2 to 6 times varying irregularly). He then placed a *test* sheet on the tray, arranged the switches so that the desired effect (*e.g.* a flash of right, of wrong, of both lights or no flash) would be given and gave the signal to hit. This hit resulted in the effect arranged for. The signal for another hit was then given. The test sheet would then be covered with the 2nd *practice* sheet.

By this means there was a record of the two successive hits (*i.e.* the one concerning which the information was given and the next hit). The influence of the information would be indicated by how close the 2nd hit came to the first. The distances were measured in millimeters by an ordinary rule.

Results

The measurements for the different types of signals, based on 17 high school students, are given below:

THE EXTENT[5] TO WHICH A SUBJECT TENDS TO REPEAT A HIT AFTER BEING GIVEN DIFFERENT TYPES OF INFORMATION

Type of Signal	Right	Both	Wrong	Control (No Information)
No. of Subjects	17	17	17	17
Total No. of Pairs of Hits	51	49	52	51
Mean Distance between Hits (in mm.)	23.2±2.0	34.7±2.9	39.5±2.7	57.6±3.7
S.D.	21.5	30.6	28.4	39.7

It should be pointed out that the S.D. and P.E. may not be strictly applicable here insofar as we have in each distribution 3 observations from each of 17 subjects. I give them as *some* indication of the unreliability of the measures.

With the same caution in mind we may now look at the differences between the different methods of signalling. In the table the differences are placed in the column headed by the signal having the larger deviation. That is the differences show a greater deviation for the signal in whose column they fall. All the differences in the column headed control indicate that the value for the control was greater than the value with which it is compared.

	Both	Wrong	Control
Right	11.5 ± 3.5	16.3 ± 3.4	34.4 ± 4.2
Both		4.8 ± 4.0	22.9 ± 4.7
Wrong			18.1 ± 4.5

From these tables we see that the deviation in the control tests was greater than the deviation in any other test. That is to say *any* signal given seemed to have the effect of causing the next response to be closer to the first than it would have been had no signal been given.

[5] This is really an inverse measure of the repetition of a hit. A repetition would secure a value of 0. Any value greater than 0 indicates failure to repeat.

We see also that neither a signal of 'both' nor of 'wrong' brings the next response as close as does a signal of 'right.' Insofar as the P.E. concept applies here, these differences are all dependable.

The difference between 'both' and 'wrong' is not at all dependable here, but is in line with all the differences of experiment II.

Comparison with Experiment II

In comparing the above data with those obtained from a similar group (High School students) recorded by means of squares, we find a certain amount of disagreement. The more adequate measurement shows that a signal of 'Right' has more of a stamping-in influence than has the neutral signal of 'Both,' whereas the previous technique had shown a contrary tendency.

In order to check this discrepancy further an attempt was made to reduce the measured differences of responses to the records we would have obtained by using the squares. This was done by considering that any deviation of more than $\frac{1}{2}$ inch (13 mm.) would have remained in the same square. Such an assumption is only valid in general, or on the average.

By means of this conversion we find the percent of hits returning to the same place for different signals to be as follows: Right 45 percent, Both 33 percent, Wrong 20 percent, Control 4 percent.

These results show a remarkable consistency with those of the *adults* reported in experiment II.

Interpretation of Results

To summarize, we may say that in this type of experiment, any signal no matter whether it tells the subject that he is right or that he is wrong or tells him nothing, tends to stamp in the connection it follows. Information that he is right is almost certainly more likely to stamp in the bond than is a meaningless signal. It is very probable that information that he is wrong is less likely to stamp in a bond than is a meaningless signal. This probability is evidenced by a con-

sistent showing in the case of three different groups rather than by the reliability of any one set of results.

Experiment IV

In all the experiments so far the signals were given by mechanical means (presence or absence of a light or shock in Experiment I, and by letters flashed by means of lights in experiments II and III). It seemed desirable to compare this method with that of verbal signals. Accordingly a modified form of the experiment was repeated with another group of High School students. In this experiment the same sheet of directions as in experiment II was given but just as the subject sat down he was informed that the lights were not working and that instead of a light flashing, he would be told the results.

During the course of the test the experimenter said 'Right' or 'Wrong' when those signals were called for. When the signal of 'Both' was called for, the subject was told to 'Hit a little harder.' This provided the factor of some neutral happening which should convey no information comparable to 'Right' or 'Wrong,' and yet should not make the subject doubt the sanity of the experimenter.

Results

The results for 11 high school students are given below.

	Right	Hit Harder	Wrong	Control
Average Distance between Pairs....	29 ± 2.6	35 ± 2.5	72 ± 3.9	49 ± 3.6
S.D.....	22.5	21.4	32.8	30.7

Differences

Hit Harder minus Right.....	6.0 ± 3.5
Control minus Hit Harder.....	14.0 ± 4.4
Wrong minus Hit Harder.....	37.0 ± 4.6
Wrong minus Control.....	23.0 ± 5.3

Comparison with Experiment III

Since this experiment differed from experiment III only in the substitution of spoken words for the signals of lights, it may be interesting to compare the two results.

	Right	Neutral (Both Lights or Hit Harder)	Wrong	Control
Experiment III Signals by Lights...	23.2 ± 2.0	34.7 ± 2.9	39.5 ± 2.7	57.6 ± 3.7
Experiment IV Signal by Spoken Words.....	29 ± 2.6	35 ± 2.5	72 ± 3.9	49 ± 3.6
Difference.....	5.8 ± 3.3	0.3 ± 3.8	32.5 ± 4.7	8.6 ± 5.2

The only information whose influence seems markedly affected by the means of signalling is 'wrong.' Telling a person he is right by word of mouth seems about the same thing as flashing a light with the same meaning. Telling a person to hit harder has about the same influence (and that a stamping-in influence) as flashing a meaningless arrangement of lights. *Telling a person he is wrong, however, tends to stamp out the bond, whereas flashing a light which means 'wrong' tends to stamp in the bond.*

The latter assertion is borne out by the fact that in experiment III the responses followed by 'Wrong' (light flashing) were more nearly repeated than those followed by nothing (control), whereas in experiment IV the responses followed by the *word* 'wrong' were less nearly repeated than those followed by nothing (control).

Further Check on Experiment IV

This fact appeared so striking and so anomalous that I attempted to check it up with other groups. In this check the procedure was less refined. The subjects sat at an ordinary table and were blindfolded in the ordinary way. In place of the large arm rest which the subjects in the preceding experiments had used, a small typewriter ribbon box was substituted on which the subjects rested the pencil. Some subjects reported that the effort involved in finding this box after each hit distracted their attention from the main task. The directions were given verbally as the blind fold was being adjusted. The signal to hit varied from a tap on the subject's left arm to a click of a sounder. The experiments were conducted by 3 different people.

In view of all these points in which the technique was altered, the results of this check must not be credited with

too much independent validity. I present them merely to show their bearing on the seemingly anomalous data obtained from the 11 high school subjects. In order to facilitate the comparison I repeat the results of the high school students, and also average the results of all subjects.

The results from the college students and stenographers also indicate that the hits following a statement of 'Wrong' deviate more widely than the hits which follow no statement. They also indicate that hits following a statement of 'Hit Harder' deviate less than the control hits. The slight and uncertain difference between the influence of 'Right' and of 'Hit Harder' found in the High School Students is also borne out by the fact that in the college group the values for these two signals were practically identical.

Discussion of the Discrepancy Between Experiments III and IV

I have as yet no definite explanation of the fact that flashing a light to tell the subject that he is wrong stamps in the bond, while just saying 'wrong' stamps out the bond. I suspect, however, that the discrepancy is due to the fact that in the case of the words the significance of the signal is more to the fore than is the physical medium by which it is conveyed. When the word 'wrong' is uttered, it is not likely that the subject responds first to the fact that the experimenter is speaking and secondarily to the fact that this means he is wrong. He is, instead, likely to respond entirely to the fact that he is wrong. When the 'wrong' light is flashed, however, I suspect that the subject does respond primarily to the fact that a light is flashing and later to the fact that this signal means wrong. Since a neutral stimulus does have a stamping-in effect, it does not seem too much to suggest that in the case of the light there is an opportunity for the stamping-in physical medium of the symbol (light flashing) to overweigh the stamping-out influence of the meaning of the symbol.

Bearing on Other Studies

In comparing these results with the data of the other studies we must remember some striking differences in the

Group	No. of Subjects	'Right'			'Hit Harder'			'Wrong'			Control		
		No. of Hits	Total Dev.	Av. Dev.	No. of Hits	Total Dev.	Av. Dev.	No. of Hits	Total Dev.	Av. Dev.	No. of Hits	Total Dev.	Av. Dev.
High School	11	33	955	29	33	1,153	35	33	2,364	72	33	1,603	49
College	16	48	1,120	23	48	1,061	22	48	1,961	41	48	1,222	25
Stenographers	8	24	443	18	23	747	32	24	1,576	66	24	878	37
Combined groups	35	105	2,518	24	104	2,961	28	105	5,909	56	105	3,703	35

procedure. In this study we have a stimulus, a response, an effect *and immediately thereafter a repetition of the stimulus.* In the other experiments this repetition ordinarily did not come until 24 hours later, during which time some 200 other stimuli were presented. Moreover in the other experiments every response was followed by some effect. In my experiments only a few (about 1/6) of the responses were followed by an effect.

The other studies found that a verbal statement of 'wrong' had practically no effect or if any a stamping-in effect. In my experiments such a verbal statement of 'wrong' had a distinct tendency to stamp-out the bond. It is altogether possible that the discrepancy is due to the difference in the intervals between the incidence of the effect and the next presentation of the stimulus.

General Conclusions

In attempting to account for the stamping-in influence of punishment, we find first the interesting fact that punishment of the kind here used has no uniform stamping-in influence but that its effects vary with the conditions. When conveyed by one medium (flashing lights) punishment stamps the bonds *in* as shown by an immediate test. When conveyed by another medium (a spoken word) punishment stamps the bond *out* as shown by an immediate test. Thorndike found, however, in a test delayed for 24 hours, that punishment conveyed by spoken words had practically no effect. Consequently we may conclude tentatively that the influence of punishment varies with the medium by which it is conveyed and with the time elapsing between the occurrence of the punishment and the next presentation of the stimulus.

Our main problem is concerned, however, with the suggestion that whenever punishment does strengthen a bond, such strengthening may be due to the incidence of the physical medium by which the punishment was conveyed. We suggested that the mere fact that 'something' happens after a response tends to strengthen the underlying connection, and that a rewarding something may add to the strengthing in-

fluence and a punishing influence detract from it. This we find to be precisely the case. When a signal carrying no information of success (but not ridiculous) is given it has a decided stamping-in effect. When practically the same signal is made to inform the subject that he is right, the strengthening value is enhanced. When the signal informs the subject he is wrong, the strengthening influence is diminished.

In view of this the effect of punishment no longer appears to be anomalous. When measured from the base line of "informationless something happening," punishment and reward seem to have distinctly opposite (if not equal) effects.

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FACTORS INFLUENCING THE LATENT TIME OF THE PATELLAR REFLEX *

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Since its discovery independently by Erb (**6**) and Westphal (**36**) in 1875, the study of the patellar reflex or knee-jerk has been an actively cultivated and fruitful source of experimental data relative to the nature of reflex action. The history of the concept of reflex action has been carefully presented by Hall (**15**), Hodge (**16**), Gault (**14**), Sternberg (**26**) and Fearing (**7**, **8**) and most recently by Skinner (**25**). No useful purpose would be served here in even briefly reiterating the historical background. It will suffice to point out that through the work of Descartes, Swammerdam and Hall the basis was provided for a physiological explanation of action. The satisfactory solution of the Pflüger-Lotze controversy by demonstrating that reflexes really were predictable as Hall had stated and thus obviating the necessity for Pflüger's 'spinal soul' opened the way for Magnus and Pavlov who have carried the reflex arc concept to its logical conclusion in setting it up as the chief explanatory principle of behavior. The introduction of the 'neurone theory' by Waldeyer (**34**) in 1891, and the actual use for the first time by Foster (**9**, p. 329) of the term synapse, completed the necessary physiological basis on which to build such a system. The fact that the reflex arc concept gave a strong neurological foundation for association psychology and the ease and clarity with which it could be 'diagramed' in textbooks, accounts for its well-nigh universal acceptance. The reflex arc concept, together with its accompanying doctrine of the synapse as the locus of conditioning has not however, advanced unchallenged to its present dominant position.

John Dewey (**2**), by 1896, had sounded a formal protest against the uncritical use of 'the reflex arc concept in psychol-

* From the laboratory of the University of Southern California.

ogy.' Loeb, working chiefly with lower forms of life, had come by 1903 to the conclusion that (**20**, p. 5):

> The irritable structures at the surface of the body and the arrangement of the muscles, determine the character of the reflex act. The assumption that the central nervous system or the ganglion-cells are the bearers of the reflex act cannot hold.

Loeb's contention that the nervous system is merely an efficient 'protoplasmic bridge' between receptor and effector is most basic as physiological support for the revolt issuing out of the experimental results obtained by Franz (**10**, **11**) and Lashley (**18**), in one field and by Tuttle (**28**, **29**, **30**), Emery (**5**) and Freeman (**12**, **13**) in another. The basic protest underlying these and many other studies has achieved at least one major systematic presentation in Gestalt psychology. Some exponents of Functionalism as a school would doubtlessly feel that their views represent perhaps an earlier protest of this sort.

It is, as stated above, because the knee-jerk offers such an excellent approach to these basic problems that it has been an almost constant object of experimental study since its discovery. The vast majority of all the work done on the knee-jerk has, however, concerned itself with analyses of the factors influencing its extent.

The technical difficulties involved in determining the latency of the knee-jerk are probably chiefly responsible for the relatively small amount of data that has been accumulated in this field as compared with that available on the extent of the jerk. So far as is known there has, heretofore, been no means available by which measurements of latency could be made in a fashion even remotely approaching the speed and facility with which they can be made on extent.

Another unfortunate circumstance from the standpoint of our present interest is the fact that much of the finest of the early work on latency was directed toward a solution of the question as to whether or not the knee-jerk was a true reflex. Out of this early interest has grown the press of the best workers to cut down the latency period, not merely to the point of its actual duration as a reflex through elimination of mechanical errors, but to eliminate as far as possible, latency

of muscular contraction, in short, to attempt to determine the conduction rate of the reflex arc. This is, of course, intrinsically of great value and we shall use these results later in this paper, but it has distracted attention from another and perhaps more pertinent field for us, for as Fearing has said (**8**, p. 255):

> The investigations which are of particular significance to the history of physiological psychology are those which study the reflex as a functional part of the intact nervous system, and not as an artificially isolated and simplified unit.

It was with this feeling, that what is most needed now is the accumulation of a large amount of data relative to the influence of specific and isolatable factors upon the latency of the knee-jerk, that the writer undertook the present investigation. The splendid work on extent of Bowditch and Warren (**1**), of Lombard (**21**) and especially of Tuttle and his associates sets the standard for work on latency.

Procedure

The apparatus for the present study centers around a new type of high speed chronograph developed by the writer to meet the technical difficulties which have already been suggested. This new chronograph has been described in a previous number of this Journal (**33**) so that it will be unnecessary to repeat the description here. It should be noted, however, that the record takes the form of a clear straight line traced upon a sensitized paper. These lines are long enough (1.26 mm. for each thousandths of a second of elapsed time) to obviate the danger of inaccurate reading of records, a possible source of error that has vitiated the results obtained through certain other techniques.

The chronograph is connected in series with a control key and stimulus and response keys, as shown in Fig. 2 of the original description. The stimulus key is made an integral part of the stimulus hammer. It is so arranged as to be closed instantly by the blow on the tendon. The stimulus hammer, which weighs 350 grams, hangs freely as a pendulum from a framework. It is allowed to fall through an arc of 45 degrees. The framework also supports a screen which prevents the subject from seeing the apparatus or catching cues as to the moment of the release of the hammer.

The response key consists of a telegraph key attached to a frame which holds it in a vertical position behind the subject's heel. The weight of the subject's foot as held slightly forward of perpendicular is sufficient to keep the circuit closed until the incipient movement of the kick. Both keys are adjustable to fit any subject.

The experiment was carried on in a small room assuring protection from outside disturbances. All subjects were members of beginning classes in psychology in their first or second year in college. Subjects were uniformly instructed (except in measurements with bodily tension) to relax and in so far as possible to think of other things than the experiment. Subjects on whom serial measurements were made appeared according to a uniform schedule during their period of testing. Handedness of the

subjects was determined by means of a battery of the usual tests, namely: eyedness, tapping, ball tossing, needle threading and paper cutting.

The Data.—The main group of data consists of over 10,000 measurements of knee-jerk latencies made with 36 subjects, of whom 16 were men. Twenty-one were right-handed and 15 left-handed.

The procedure was to obtain from 10 to 20 measures each from the right and left knees both relaxed and with facilitation. The number of days on which such measures were made is indicated for each subject in the table summarizing the results.

Table I is a general summary of the measures of all subjects. The mean latencies of left-handed and right-handed subjects are listed separately as well as the means of all the cases. In spite of wide individual variations, which are a characteristic of all measures of knee-jerk latency, the means show, as would be expected with as extensive a group of data as this, a very small probable error. In this regard it is also important to note that the means of all the right legs and of all the left legs are the same, and that the means for right tension and left tension, although varying five and six sigma respectively from the right and left under relaxation, vary only one sigma between themselves.

Relation of Handedness to Knee-jerk Latency

Perhaps the most basic problem related to handedness is the question as to whether it is an innate or an acquired trait. It appeared to the writer that the knee-jerk would be an excellent response to use in examining into the nature of handedness. The knee-jerk is certainly a native response and one which quite certainly will not have been influenced by training. If handedness is part of an innate lateral superiority either in conduction rate or coördination then the patellar reflex should show it. If it is an acquired condition, as held by Watson (**35**), then no significant differences would be expected. If the theory of cerebral dominance, as advanced by Orton (**24**) is correct and one hemisphere takes the lead in giving the side it controls the superiority in bilaterally coördinated movements, then should we also expect a difference in reaction times unless it be held that only the higher or acquired coördinations are thus controlled.

An adequate group of data relative to the latencies of the two knee-jerks of both right and left handed subjects should provide basic evidence as to the adequacy of the several theories. A survey of the literature shows no previous approach to the problem of the relation of handedness to the latency of the intact patellar reflex.

TABLE I

SUMMARY OF DATA

(IN SIGMA)

Left-handed Subjects

Subj.	No. Days	Right	Right Tension	Left	Left Tension
Si	6	55	52	60	55
Sh (m)	4	54	54	55	48
Ro	7	64	58	51	47
Ma	11	53	55	49	48
Fr (m)	9	57	54	51	52
Sr (m)	4	59	58	58	56
Ki	4	73		72	
La	6	67		68	
Fl	6	66	58	69	61
To	1	69	65	64	76
Sm (m)	1	63	60	67	62
Lo	1	66	63	57	61
Ja (m)	1	73	69	60	56
Pa (m)	1	48	50	49	50
Wi (m)	1	30	33	37	37
	Means	60	56	58	54
	S.D. mn	9.5	8.9	7.7	11.5
	P.E. mn	0.81	0.82	0.66	1.07

Right-handed Subjects

Subj.	No. Days	Right	Right Tension	Left	Left Tension
Gw	34	50	48	54	49
Na	11	51	49	49	48
Co	5	60	61	66	61
Di	4	47	53	50	49
Be	2	69	69	70	69
Bu	7	60		60	
Ec	9	65	67	63	62
Sc	4	72	66	76	66
Ko (m)	1	56	65	69	69
Bo (m)	1	78	79	98	86
Wa (m)	1	64	53	58	48
Bud	1	62	63	72	65
Bur (m)	1	87			
Hu (m)	1	70		73	
Sm (m)	1	82		87	
Da (m)	1	89		85	
Sw	1	61	54	52	52
vG (m)	1	55		65	
Wo (m)	1	71		68	
We	1	57	55	63	55
Wa	1	78	80	84	72
	Means	66	61	68	61
	S.D. mn	11.1	12.9	13.1	9.6
	P.E. mn	0.79	0.96	0.93	0.72
	Means of ALL Cases	63	58	63	57

In the present study over 5,000 latencies of the right patellar reflex were compared with a similar number of the left. Equal numbers of records from right and left knees were taken at each experimental period. Records of right and left knee-jerks, taken under conditions of bodily tension were also compared.

Results on Handedness.—Table I, summary of results, shows an absence of any significant relationship between handedness and the knee-jerk latency of the two legs. A study of the individual subjects shows that right and left handed subjects have average latencies that are either the same or that vary in an approximately chance order.

In terms of mean latencies we find that the left-handed cases show a difference of 2 sigma, both relaxed and under tension, in favor of the dominant side. The probable errors of these means vary, however, from 0.66 to 1.07 sigma so that there is actually a significant difference of only slightly more than .5 sigma between any of these measures.

In the right-handed cases the significant difference between the right and left under tension is only .28 sigma whereas the means for the right and left legs under tension are exactly the same! These similarities take on an increased significance in view of the marked systematic differences due to tension.

Whether differences in latency are considered due to differences in reflex arc conduction rate, or to a generally superior condition or coördination of muscles, handedness in its usual meaning as a native trait should result in a fairly uniformly decreased latent period for the favored side. Since the present results show no systematic difference between handedness and latency of the knee-jerk they may be considered to be in opposition to the concept of handedness as a general condition of lateral superiority in function.

The Relation of Bodily Tension to the Latent Time of the Patellar Reflex

The influence of bodily tension upon the extent of the knee-jerk was one of the earliest phenomena studied subsequent to its discovery in 1875. Jendrassik (**17**) pointed out

that frequently the kick could be elicited if the subject clenched his fists at the same time that the blow was administered, whereas the same blow would not elicit a response during relaxation. The well-known 'Jendrassiks Handgriff' thus took its place as a basic phenomenon in this field. Interest in the 'Handgriff' was further enhanced by the observation that the degree of reinforcement was dependent upon the exact time relations between the onset of tension and the moment of stimulation. It remained for Bowditch and Warren (**1**) in one of the most complete studies yet made on the knee-jerk to establish the precise limits of these relationships.

Even prior to this work Mitchell and Lewis (**23**) had established the point that the reinforcement was not produced by any specific process, but that any concomitant bodily process will produce this result. Their interesting observation of this point follows:

> Wherever we can get such (directly stimulated muscle) movements, . . . every distinct muscular exertion, even as slight as winking, increases the jerk, if it be timed correctly.

It is clear that we have here a phenomenon that may have most basic implications for the whole problem of tonus. It is for this reason that such a great number of the studies on extent have dealt with various phases of this problem. Tuttle (**28**, **29**, **30**, **31**), in the most complete series of studies on knee-jerk extent that has been made in recent years, has shown (**30**) that mental activity increases the extent of the kick. These and other studies by Miles (**22**), Dodge (**3**), Emery (**4**, **5**) and others seem essentially to be elaborations and verifications of the generalization made by Lombard (**21**, p. 70) in 1888:

> In general, then it may be said that the knee-jerk is increased and diminished by whatever increases and diminishes the activity of the central nervous system as a whole.

Because of the great systematic interest attaching itself to this problem, the writer was surprised to find that no studies of the influence of bodily tension upon the latent period of the knee-jerk had been reported. It is most unusual

that this interesting correlate of Jendrassik's study on extent should have been ignored for so long in such an active period of study.

In order to determine the influence of this widely studied 'Handgriff' on knee-jerk latency 5,000 measurements under conditions of relaxation were compared with a like number under conditions of bodily tension. The procedure was to obtain measures with relaxation followed immediately by a like number of measures under tension, the latter being induced by instructing the subject to squeeze 'as hard as you can' with both hands upon a round stick one inch in diameter which was held loosely in both hands. The subject was instructed to squeeze upon the command 'squeeze' from the experimenter. This signal was given simultaneously with the release of the hammer. The subject was instructed to relax each time after the blow had been struck.

Results on Tension.—An examination of the main group of data shows that for left-handed subjects bodily tension results in a mean decrease in latency of 4 sigma each for the right and left knee-jerks. In right-handed subjects the right knee shows a mean decrease of 5 sigma and the left knee a mean decrease of 7 sigma as a result of tension.

The means of all cases show that the right leg with tension has a latency of 5 sigma less than the normal time of 63 sigma. The mean latent period for all left knee-jerks is 63 sigma under relaxation as compared with 57 sigma under tension. In view of the fact that the probable errors of these means exceed one sigma in only one case, the results would seem to establish the fact that *bodily tension results in a shortening of knee-jerk latency.*

Figure 1 shows measures of extent in which the lower measures (2) were taken under conditions of relaxation and the row above marked 3(T) were taken immediately afterwards using the 'Handgriff.' In this figure the familiar effect of tension on extent is shown in the more than doubling of the length of lines together with a marked increase in their uniformity. We have here visual evidence that there actually is being obtained the facilitating effect of the 'Handgriff.'

The fact that the facilitation is in this record accompanied by a mean decrease in latency of nearly 10 sigma provides additional evidence for the generalization that bodily tension results in marked reductions in the latency of the patellar reflex.

The present data show that in 10,000 measures, simultaneously induced motor activity resulted in a mean reduction of reaction time in the knee-jerk of 5 sigma for the right leg

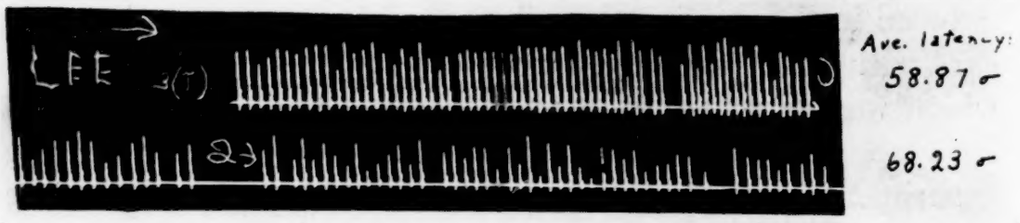

FIG. 1. Effect of tension on extent and latency.

and 6 sigma for the left. It seems strange that this interesting parallel to the 'Jendrassiks Handgriff' should have waited so long for its experimental investigation.

THE RELATION OF LATENCY TO THE EXTENT OF THE PATELLAR REFLEX

Results brought forward in the preceding sections have strongly suggested that knee-jerk latency rather than being controlled by the neural arc is more likely determined by the factors controlling the general bodily state. It has appeared possible that latency is controlled by the same factors which control the extent of the kick. The present results constitute the findings of an experimental attack on this problem.

From each of 7 subjects, 20 to 80 simultaneous measures of extent and latency were made, both under conditions of relaxation and of bodily tension. This is perhaps the first time that simultaneous measures have been so compared and by this method facts are brought to light which seem to have far-reaching implications.

The apparatus used to measure the extent of the kick is a modification of the recording technique used by Tuttle

(**27**). In this procedure a strong thread is attached to the subject's heel from where it acts upon a reduction pulley and finally actuates a stylus on a kymograph drum. This device makes possible the recording of as many as 1,000 knee-jerk extents upon one sheet of paper. An example of such a record is shown in Fig. 1 which is the record of subject Le shown in Table II.

TABLE II

RELATION OF LATENCY TO EXTENT BASED ON SIMULTANEOUSLY MADE MEASURES DURING RELAXATION AND TENSION

Subj.	Relaxation		Tension	
	Latency (σ)	Extent (mm)	Latency (σ)	Extent (mm)
Na	76	1	55	7
Bu	94	2	67	7
Hu	65	7	53	12
Sm	100	3	81	8
Wo	64	8	56	10
Da	95	4	69	11
Le	68	6	59	10

$r = -.90 \pm .041$ $r = -.77 \pm .081$ $r = -.61 \pm .137$

In the present group of cases, as shown in Table II, there is an inevitable increase in extent accompanied again inevitably by a decrease in latency, as a result of bodily tension. Since there are no exceptions to these findings we may generalize from them that knee-jerk latency varies inversely with its extent.

A comparison of subject Bu with subject Wo, for example, also shows that while the relation is probably not proportional, yet the greater variations in extent are accompanied by more extreme changes in latency. This point is made clearly evident in connection with the second approach to the present problem.

The results of a slightly different attack on this same problem are shown in Fig. 2. This figure presents the results of 450 simultaneously made measures of latency and extent. These 450 measures were made at one experimental sitting, being run at the rate of about 5 a minute. The consistent increase in latency from 66.83 sigma when the subject was

fresh to a mean latency of 78.70 sigma at the close of the session is shown in the figure to be the direct accompaniment of a decrease in the extent of the kick. We here see that it is not the 'Handgriff' as such that influences latency but that

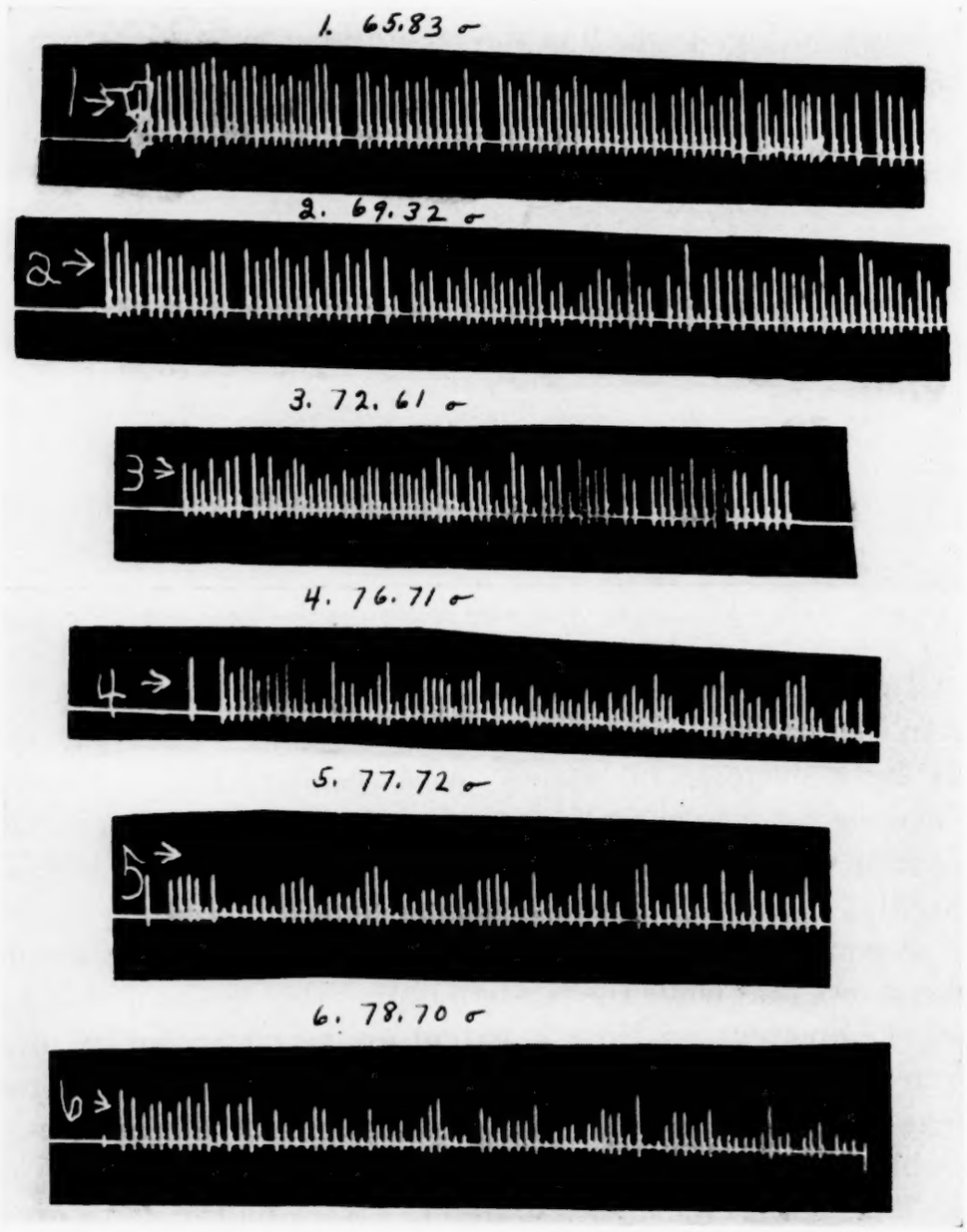

FIG. 2. Inverse changes in extent and latency with repetition.

any other factor that affects muscle tonus affects both latency and extent. The validity of this generalization is supported by the results of the present study as to the effects of bodily tension upon latency.

Although the means of the individuals in this study show a perfect negative correlation between latency and extent, inasmuch as extent was inevitably increased and latency inevitably decreased by bodily tension, it was decided, in view of the important implications of the results, to carry the investigation further.

In the first place it was thought desirable to see how well the group as a whole followed the tendency of the individuals. Determining the correlation of latency with extent for the group under relaxation we found the very high negative correlation of $-.90 \pm .041$. Under tension the correlation is lower but very important, being $-.61 \pm .137$. For both series the negative correlation is $-.77 \pm .081$. These correlations, especially in view of their small probable errors, indicate the high degree of reliability of the present results.

In order to still further validate the bases for our generalization and also to provide an experimental approach to the question as to whether this relationship resulted from the condition of bodily tension or whether the bodily tension was simply one of many causative factors, comparisons of individual records of both latency and extent were made in such a way that the measures of extent and latency of any given kick could be compared.

Although somewhat difficult of achievement, it has been possible to obtain 156 simultaneous measurements from 5 subjects in such a way that each measure of latency can be compared directly to its measure of extent. These measures taken under conditions so as to result in somewhat marked differences in extent, are presented in tabular form in Table III. Subject Hu, Fig. 3, shows well the typical situation in which the 2 variables are seen to change almost invariably in an inverse sense. Not only are the changes almost universal, but they are quite similar in their proportionate degree of change.

The present results, it is felt, show conclusively that variations in extent of the kick are accompanied by inverse changes in the latency of the kick. We may therefore state that whatever complexity of factors controls one also controls the other

TABLE III *

RELATION OF LATENCY TO EXTENT

Comparison of Simultaneously Made Measures

	Subject v.G.		Subject Sh		Subject Hu		Subject Le		Subject Wo	
	Latency	Extent	Latency	Extent	Latency	Extent	Latency	Extent	Latency	Extent
1	60	13	73	12	64	10	65	9	70	9
2	56	14	64	11	65	10	65	8	66	8
3	58	16	60	10	80	2	65	8	68	7
4	59	12	60	11	69	6	65	8	62	8
5	66	10	72	12	72	5	69	7	67	8
6	66	7	60	11	65	9	70	7	64	8
7	71	4	74	9	63	10	70	6	63	8
8	53	16	68	11	82	1	70	6	69	5
9	50	14	60	11	84	1	74	5	69	5
10	55	14	74	10	70	5	75	5	67	6
11	62	13	63	11	81	1	77	5	63	6
12	55	14	68	10	65	10	78	3	69	7
13	59	10	85	4	67	10			80	2
14	62	13	83	3	63	10			75	3
15	56	13	74	6	69	8			78	2
16	56	14	76	6	61	12			85	1
17	62	9	80	3	81	1			65	8
18	66	4	61	7	76	3			62	9
19	53	12	77	4	78	2			64	8
20	71	6	60	11	70	6			70	6
21	61	6	60	11	81	1			68	5
22	56	14	60	10	79	1			66	8
23	52	14	65	11	66	9			68	6
24	62	13	78	5	64	10			74	3
25	59	12	71	8	65	10			96	2
26	57	14	82	4	69	6			75	3
27	59	14	74	4	69	5			78	2
28	52	13	69	7	69	7			72	3
29	54	15	95	1	69	6			65	9
30	54	14			88	1			66	9
31	54	12			84	1			64	8
32	59	10			65	8			70	5
33	62	11			89	1			78	4
34	65	4			66	7			68	5
35	66	2			65	8			65	9
36	62	10			65	9				
37	63	10			64	10				
38	69	2								
39	61	8								
40	64	6								
41	61	7								
42	69	1								
43	65	4								

* Extent in mm. as recorded on kymograph drum. Latency in sigma.

and reaffirms our principle that *knee-jerk latency varies inversely with knee-jerk extent.*

Interpretation of Results. The importance of the foregoing generalization may be readily appreciated when we realize

that there have, as already pointed out in the introduction, been very extensive studies covering the factors influencing the extent of the knee-jerk. Since knee-jerk latency varies inversely with its extent it follows that both are controlled by the same factors. This latter fact bears very far reaching implications. In the first place it means that all the information acquired during 50 years of intensive study of knee-jerk extent can now be applied to problems related to latency.

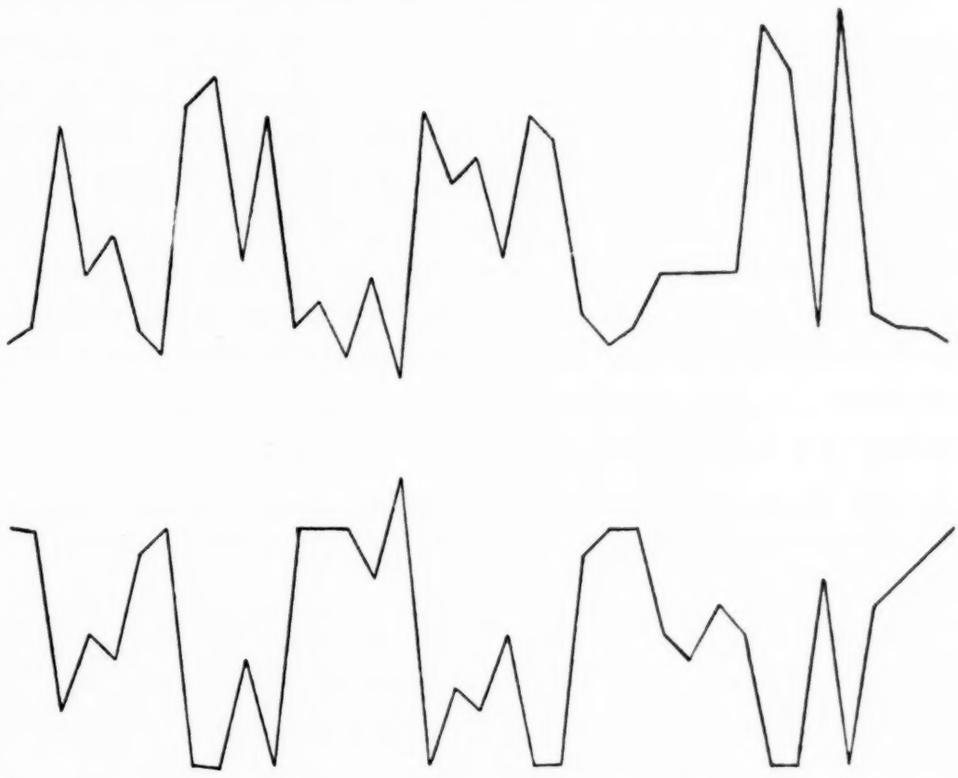

FIG. 3. Inverse relation of latency to extent. Subject Hu. (Table III.) Upper line: latency in sigma. Lower line: extent in millimeters.

The second implication, one basic for questions of systematic interest, grows out of the first. It has been thoroughly established, as already pointed out, that knee-jerk extent is determined by those factors which determine general bodily tonus. Indeed, in recent studies the knee-jerk has been frequently used as a gauge of bodily tonus.

We have in the foregoing, it would seem, evidence of the strongest kind for the contention that has been emerging throughout the present study. Knee-jerk latency varies

with the general bodily state and therefore its changes can not be due to changes within the neural arc itself.

It is the present contention that the results of this study, together with those of Tuttle and Brown (**32**) and of Whitehorn and Lundholm (**37**), which indicate that neural conduction rate is a constant factor, render impossible the theory that reflex reaction time is controlled by factors affecting the neural pathway over which the impulse passes.

The implications of the foregoing in terms of the supposed rôle played by synapses in learning are too obvious to justify special comment in this experimental study. Suffice it to say that it seems to point back to the basic hypothesis laid down by Loeb in his concept of the nervous system as being a protoplasmic bridge between receptor and effector. As Loeb himself has pointed out, the nervous system has become greatly elaborated, but there seems to be no great necessity for the assumption that it has acquired some wholly new function either at the synapse or at other parts of the nerve cell. Lashley has indeed declared that (**19**, p. 2):

> There is no direct evidence for any function of the anatomical synapse: there is no evidence that synapses vary in resistance, or that, if they do, the resistance is altered by the passage of the nerve impulse.

The present results are offered as evidence that neither synaptic resistance, nor indeed, any specific function of a neural pathway plays a determining part in controlling the latencies of the responses here studied.

It is suggested that the great variability of both the extent and latency of the knee-jerk are explained in large part by variations in muscle tonus and that the neural arc is merely the connecting protoplasm which sets off the tetanic responses of the quadriceps, even as the same stimulus is being conducted over other protoplasmic pathways, and especially through higher integrating centers to affect thereby all parts of the body. The total extent of such outpourings determine the condition of general tonus of the body from moment to moment and thus the vigor and speed of any specific reaction which may then occur.

The interpretation of the present results points then, toward that view in modern psychology which no longer regards reaction in general as well as simple reflexes, as being controlled by specific pathways, speed of conduction over which is determined by conditions of the nerves which constitute it, but rather as being the result of all the complex factors affecting a complex organism, every reaction of which is a reaction of the organism-as-a-whole.

Summary

1. The factor of handedness bears no statistically important relation to knee-jerk latency in the present group of subjects. The differences between the means of right and left knee-jerk latencies in no case exceeded the probable errors of the 2 means by more than a fraction of a sigma.

2. A comparison of knee-jerk latency under conditions of bodily tension and relaxation showed the following averages:

Right knee (relaxed) $- 63\sigma \pm 0.80\sigma$
Right knee (tension) $- 58\sigma \pm 0.89\sigma$
Left knee (relaxed) $- 63\sigma \pm 0.79\sigma$
Left knee (tension) $- 57\sigma \pm 0.89\sigma$

3. In simultaneously made measures of extent and latency bodily tension inevitably resulted in an increase in the mean extent of the knee-jerk and in a decrease in the mean latency.

4. On the basis of these results the principle is advanced that the latency of the knee-jerk varies inversely with its extent.

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THE RELATION OF STUTTERING TO MENTAL FATIGUE

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In a series of previous studies of the relation of blocking in continuous mental work to fatigue, (1) it was found that there is a decided increase in the frequency and length of blocks, or pauses, with the onset of fatigue. Whereas a relatively unfatigued subject blocks, on the average, three to five times a minute and for periods of a couple of seconds, the fatigued subject may block as often as eight to ten times per minute, and for periods whose duration is about twice as long. The hypothesis has been advanced that this increased blocking is due to some depletion in the available energy in specific response systems, whereby the delicate balance between competing tendencies is temporarily disturbed. In this way, the functioning of depleted systems is interfered with by the greater readiness of less fatigued elements to respond, due to their relatively lower thresholds, and a resulting competition or inhibition occurs which prevents the overt response from taking place. A search for factors which aggravate the blocking tendency disclosed two factors which are also conducive to the development of rapid fatigue, namely homogeneity and competition (2). Finally, blocking was found to be characteristic of the manual performance of mental tasks as well as of the vocal. The correlation between number of manual and vocal blocks per minute is $0.63 \pm .07$, while that between the lengths of the manual and vocal blocks, in responses, is $0.72 \pm .06$ for normal subjects (2).

A search for other possible causal factors in blocking led to a study of the phenomenon in one of its most exaggerated forms, *i.e.* in stuttering and hesitancy in speech. Can such

* The present investigation was aided in part by a grant to the University of Chicago from the Rockefeller Foundation.

prolonged and frequent blocks as appear in the vocal responses of stutterers be related to the general blocking phenomenon? There are many current theories of the cause and nature of stuttering.[1] Some relate it to special factors in the vocal apparatus itself, others to central neural factors, as Travis' cerebral dominance theory, and still others to emotional factors, inferiority feelings, psychoanalytic concepts, and the like. No one of these theories seems universally applicable. Perhaps they all relate to aggravating influences rather than to the fundamental cause of stuttering. Certainly if stuttering is an exaggerated form of the blocking of normal individuals, it must have similar causal principles.

The present study was therefore undertaken to make a comparison of the blocking tendency in normal and stuttering subjects. The following questions were raised: What is the relation between the frequency and length of the blocks of normals and stutterers? How does fatigue affect the blocking of stutterers? Does the greater frequency of vocal blocking in stutterers apply also to their manual reactions?

The subjects used in this experiment consisted of 33 normals and 28 stutterers. Eight of the stutterers and twenty of the normals were students in the University of Chicago. The rest were chosen from diverse walks of life. All but two of the stutterers were men, due to the great difficulty of finding females who stuttered. In most cases the stuttering was quite severe, interfering seriously with conversational speech, and all cases were of long standing. Most of the cases had tried one or more methods of speech correction, without success. Practically all reported that they stuttered most when they were emotionally embarrassed, in social situations, and when taken unawares with insufficient time to prepare an answer. Fatigue and anxiety also aggravated the condition.

The task, chosen on the basis of previous work, was color-naming. Woodworth-Wells color charts were used for the purpose, with 5 colors. Two types of record were ob-

[1] These are reviewed in Stinchfield, S. M., *Speech Disorders.* N. Y., Harcourt Brace, 1933.

tained from each subject; a vocal record and a manual one. The exact procedure was as follows: The subject was first accustomed to the laboratory situation, given an information card to fill out, and then given preliminary practice, with directions, in responding to the colors at top speed both vocally and manually. In the case of the stutterers, if they were unable to pronounce the name of any of the colors without stuttering, some other name was substituted, so that actual stuttering in naming the colors was reduced to a minimum. The manual responses were made by pressing keys corresponding to each of the five colors, the code for which was placed in plain sight. When adequate practice had been given, the subject was put through the experiment proper in one or other of the two following orders:

Order A.

Five minutes vocal work	Five minutes manual work	Five minutes vocal work	Five minutes manual work

Order B.

Five minutes manual work	Five minutes vocal work	Five minutes manual work	Five minutes vocal work

The instructions were to work as fast as possible consistent with accuracy. Mistakes were corrected by the subject as he went along. The short work periods used did not permit a very large amount of fatigue to develop, but they balanced the practice effect on the manual and vocal performances.

Both manual and vocal reactions were automatically recorded on a kymograph tape, and these records were later analyzed to determine the number of responses per minute, blocks per minute, length of blocks in millimeters, number of responses between each block, and length of blocks in terms of average response time.

Results

1. How do stutterers and normals compare as to the frequency and length of blocks?

First, stutterers do not differ significantly from normals in average speed of reaction. When the number of responses per minute of each group has been corrected by allowing for

time consumed in blocking, a small but insignificant difference appears between them.

	Normals	Stutterers	Difference
Manual responses	72.2	69.7	2.5 ± 2.3
Vocal responses	101.8	97.7	4.1 ± 3.0

Second, in frequency of blocks, as shown by average number of responses between blocks, there is a decided difference. Stutterers block about twice as often as normals, in both vocal and manual responses.

	Normals	Stutterers	Difference
Manual responses	11.8	6.9	4.9 ± .6
Vocal responses	15.3	7.6	7.7 ± .7

Third, in length of blocks, there is a significant difference not only in terms of millimeters, but also in terms of number of average reaction times to a block.

	Normals	Stutterers	Difference
Manual blocks	5.3 M.M.	6.6 M.M.	1.3 ± .33
Vocal blocks	4.0 M.M.	5.5 M.M.	1.5 ± .32

In terms of responses, the stutterers show a 20 percent increase in length of blocks in manual performance, and a 25 percent increase in vocal performance. This increased blocking in the vocal performance was not caused by stuttering spasms as such. In the main, the stutterers were able to perform the act of pronouncing the names of the colors without such spasms; if not, other names were substituted. The blocks were mental blocks. And, of course, in the manual performance, there was no question of stuttering spasms; yet the blocking was much more frequent than normal. It is probably significant, however, that the greatest increase occurred in the vocal rather than in the manual performance.

Another difference worth noting is the greater irregularity of the stutterers' records. The separate reaction times show

much greater variability than those of normal subjects. Apparently there was a lack of rhythmicality in reacting. This irregularity of reaction time is interesting because it probably points to a fundamental defect in the stutterer, and accounts for the improvement brought about by the use of enforced rhythms, such as the beat of a metronome. Lack of rhythmicality of serial reaction seems to be characteristic of stutterers.

Figure 1 shows one-minute records of the vocal and manual performance of a stutterer and a normal subject. Each vertical bar represents one reaction, and the height of the bar indicates the reaction time. A long bar therefore represents a block. In the stutterer's vocal record, there are ten such prolonged reaction times, and in his manual record, eight. The normal record, on the contrary, shows five blocks in the vocal and four in the manual performance.

2. To what extent are the vocal and manual performances of stutterers correlated? To determine this, correlations were obtained between the manual and vocal records as to blocks per minute, responses per minute, and length of blocks in terms of responses. The results were rather surprising. Whereas the normal subjects showed correlations of $0.63 \pm .07$ between vocal and manual responses, $0.63 \pm .07$ between vocal and manual blocks frequency, and $0.72 \pm .06$ between vocal and manual block length, the stutterers on the contrary showed much lower correlations. Their vocal and manual responses correlated $0.46 \pm .11$, their block frequencies $0.15 \pm .13$, and their block lengths $0.03 \pm .13$. If this low correlation between vocal and manual performance were taken alone, it might be interpreted to mean that the speech difficulty is a defect confined to the vocal mechanisms, but against this is the fact that stutterers have a high average blocking rate in manual performance.

3. How does fatigue affect the blocking of stutterers as compared with its effect on normals? This can be determined by plotting curves for the successive minutes of work. The average decrement in number of responses is about the same for normals and stutterers, being 10 percent for manual and

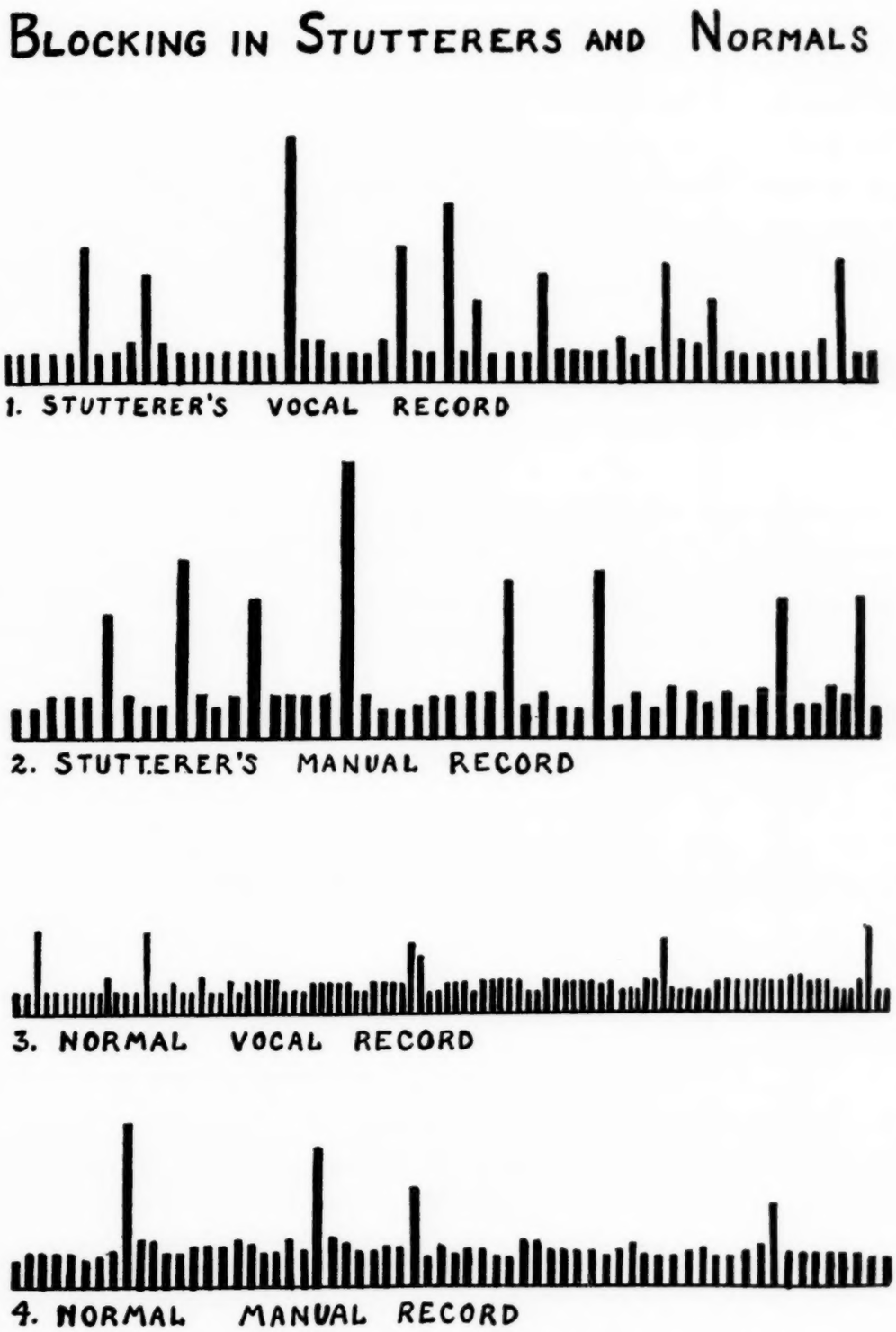
BLOCKING IN STUTTERERS AND NORMALS
1. STUTTERER'S VOCAL RECORD
2. STUTTERER'S MANUAL RECORD
3. NORMAL VOCAL RECORD
4. NORMAL MANUAL RECORD

FIG. 1. The vertical bars represent distance between responses in millimeters. Scale = ⅛ inch per millimeter. Long bars show blocks. Each record is one minute long.

13 percent for vocal responses. But the blocking frequency shows a relatively large increase in the case of the normals and almost no increase with the stutterers. This is brought out in Fig. 2. The normal manual curve has a 30 percent in-

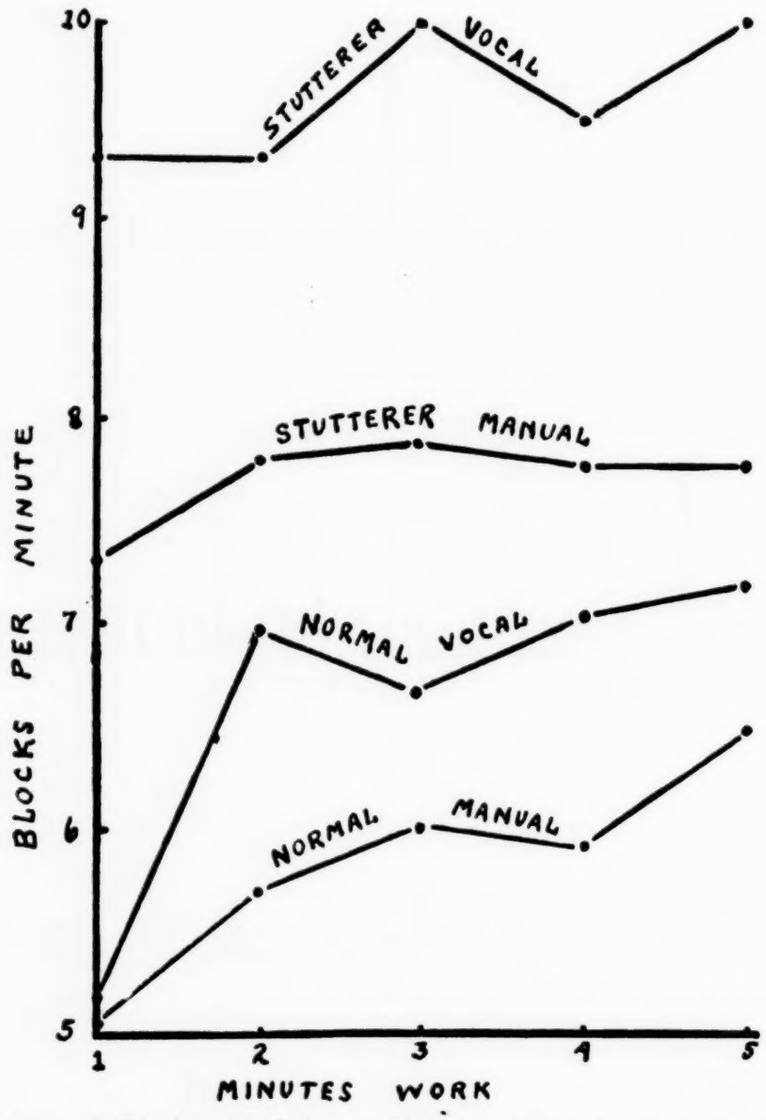

FIG. 2. Increase in blocks with fatigue. Showing the greater increase with normal subjects.

crease in block frequency, the vocal curve a 35 percent increase; whereas the stutterer's manual curve shows 7 percent and the vocal curve 8 percent increase. This may be accounted for by assuming that the stutterer begins at such a high level of block frequency that there is no room for increase, or at least that such increase is obscured.

4. How is the reaction time of stutterers related to their blocking frequency? Do stutterers who have a rapid reaction time block more or less often than those with slow reaction time? In this respect they closely resemble the normal subjects. That is, when responses per minute are correlated directly with blocks per minute, the coefficients are practically zero. But when responses per minute are correlated with number of responses between blocks, the coefficient for manual performance is $0.69 \pm .07$, and that for vocal performance is $0.62 \pm .08$ which means that the slow subjects block more frequently than the quick reactors. However, when any particular subject exceeds his usual speed he tends to block more frequently.

Length of blocks also correlates highly with reaction time. That is, the subjects whose reaction time is short have shorter blocks. The coefficient for manual performance is $0.80 \pm .04$, while that for vocal performance is $0.88 \pm .02$, or about as high as that for normal subjects. But when length of blocks is computed, not in time but in number of average response times, the correlations are lower. That for manual performance is $0.25 \pm .12$, and that for vocal $0.19 \pm .12$, which means that the slow reactor does not have longer blocks than are proportional to his longer reaction times.

Summary

The factual findings may be summarized briefly as follows:

1. Stutterers block about twice as often as normal subjects in both vocal and manual responses.

2. The blocks of stutterers are significantly longer than those of normals in both types of performance.

3. The correlation between vocal and manual performance as to frequency and length of blocks is low.

4. The blocking of stutterers does not increase proportionately with fatigue as does that of normal subjects, at least for short working periods.

5. Stutterers with slow reaction time block more frequently and longer than stutterers with quick reaction time.

Discussion

What light does this study throw on the phenomenon of blocking in general? And what does it contribute to an understanding of stuttering itself?

To begin with, it certainly establishes a relationship between blocking and stuttering. An increase in one goes hand in hand with an increase in the other. It is safe to assume that there is a common neurological basis. Moreover, since the effect includes manual as well as vocal performance, it is very probably of central origin, involving a loss of control or coördinating ability. Superficially there is a strong resemblance between the record of a stutterer and that of a normal subject who is fatigued from a long period of continuous work. There is the increased frequency and length of blocks, as well as the greater irregularity. But, of course, the fatigued normal does not show the specific stuttering spasms which characterize the stutterer's speech. He merely shows a greater hesitancy, or an incoördination which appears in the form of slips of the tongue, tangled words, etc.

One possible inference would be that stuttering is the result of a condition of nervous exhaustion at least analogous to that existing in mental fatigue. Certain facts support the inference. Most stutterers declare that their difficulty is greatly exaggerated by any unusual fatigue. They also report a marked improvement as a result of rest. Moreover there is evident in many stutterers a lack of resistance to emotionally disorganizing factors, a tendency to lose coördination under emotionally upsetting conditions. Some investigators have classified stuttering with the psychoneuroses, neurasthenia, hysteria, etc. Neurasthenia has long been considered a phenomenon of mental fatigue. The nature of this form of mental fatigue is unusual, however, because it can seldom be traced to any overwork of the mind but is rather associated with exhausting emotional conflict. The

symptoms, nevertheless, are those of fatigue—nervous exhaustion and a feeling of inadequacy.

Another inference would be that, since any condition which produces incoördination or an increased competition between response elements causes increased blocking, the factors which produce stuttering are also varied. Embarrassment, self-consciousness in social situations, etc., are known to exaggerate it. The stutterer's emotional threshold may be relatively low. This would explain the effectiveness of all forms of treatment which are aimed at a stabilizing of the stutterer's emotional life, removing inferiority attitudes, and the like. No doubt the readjustments involved in overcoming left-handedness, at about the time when speech is being acquired, create enough extra competition to establish the habit of stuttering at this age, so that the speech habit is defective from the start, and later emotional conflicts exaggerate it.

In searching for possible ways of reducing the frequency of blocking in stutterers, it was found that, when the rate of presentation of the stimuli was automatically controlled, so that the subject had to keep up, less blocks appeared than when the subject was allowed to voluntarily govern his rate, and was merely told to work as fast as he could. In fact, the stutterer could far exceed his voluntary rate and still block less often. Since the automatic rate involved the ticking of a metronome, it was not certain whether the reduction in blocking was due to the advantage of the rhythm in regulating and coördinating the responses, or whether the sound of the metronome tick furnished a reinforcing stimulus. Both inferences seemed plausible, since the stutterer suffers from a lack of coördination, on the one hand, and an insufficient energy of motor discharge, *i.e.* a hesitancy to initiate responses, on the other. Therefore, the loudness of the metronome tick was varied within wide extremes, and a condition was set up in which the automatic rate of presentation of the stimuli could be controlled noiselessly so that there was no auditory reinforcement. It was found that both factors contributed something to the reduction of blocking. The entire problem of voluntary versus automatically controlled

rates has therefore been made the subject of a special investigation now being carried on.

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TONAL DENSITY

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One of the *O*s in Rich's [1] experiment on tonal volume reported, "The larger [lower] tones seem to have a greater diffuseness. It seems as if the smaller [higher] tones fill space more compactly, what they fill." Similar characterizations of the phenomenal aspects of tones have appeared in the protocols of several *O*s in more recent experiments. An *O* in an experiment performed by Gundlach and Bentley [2] described high tones as 'denser' and 'compacter' than low tones. Likewise, in Moul's [3] experiment, where she sought to obtain phenomenal descriptions of the 'thickness' or volume of tones, one finds that a noise was characterized as 'tight, hard, impenetrable' by an *O* who described a tone of 700 cycles thus: "The tone somehow carries the meaning of 'giveyness,' of penetrability, of a certain looseness, a certain softness in the sense that a vapor is soft." Another *O* in the same experiment experienced a tone of 1000 cycles as 'thick, dense, compact.' The same tendency of *O*s to experience high tones as dense, hard, compact, impenetrable was present in the author's [4] experiment on tonal volume and suggested that density might be a discriminable aspect of tonal stimuli, different from volume, pitch and loudness. The present experiment was undertaken to test this possibility.

The *O*s used in the experiment were Dr. J. Volkmann (*V*), Mr. E. B. Newman (*N*), Dr. J. G. Needham (*Ne*), and Mr. H. S. Odbert (*Od*). *V* and *N* had observed in the experiment

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[2] R. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, *Amer. J. Psychol.*, 1930, 42, 538.

[3] Emeline R. Moul, An experimental study of visual and auditory 'thickness,' *Amer. J. Psychol.*, 1930, 42, 551 f.

[4] S. S. Stevens, The volume and intensity of tones, to be published in *Amer. J. Psychol.* The apparatus used in the present experiment is described in this paper.

on tonal volume and were well trained in the technique used in that and in the present experiment. Furthermore, they had both described tones as being more or less dense at one time or another. *Ne* and *Od* had never heard of tonal density and had, therefore, to be trained in its recognition. The training was accomplished by presenting them with a high tone followed by a low tone and asking them to notice the difference in the density of the two. They immediately reported that the higher one was more dense. They were then presented with a loud tone followed by a weak tone. In this case they both described the loud tone as being more dense than the weak tone. Their judgments thus fell into line with the reports of *V* and *N*.

The experimental procedure was to present alternately two tones of different pitch and to require the *O* to equate them in density by changing the intensity of one of the tones. This result was achieved by the use of an oscillator (General Radio, type 508-A) in a circuit so designed that by means of a switching device [5] two tones of different pitch could be presented alternately at the rate of about 40 per min. One of the tones was of fixed intensity, whereas the intensity of the other could be changed by a small variable resistance held in the hands of the *O*. The *O* adjusted the intensity, taking as much time as he desired, until the two tones seemed equal in density, and the voltage was read directly from a voltmeter connected across the output of the oscillator. Two Jensen "Concert Junior" dynamic speakers were used as the source of sound. These speakers were mounted close to the head of the *O*, one over each ear. The observing was done in a sound-room lined with a 2-in. blanket of rock wool designed so as to minimize the reflection of the sound by the walls.

Each of six comparison tones was equated to the standard tone (500 cycles at 60 db above threshold) in both density and intensity. Equations were made in respect of both aspects during each of five observational periods by each *O*. The difference between the value at which a given tone was judged

[5] S. S. Stevens, A switch for the presentation of electrically generated tonal stimuli, to be published in *Amer. J. Psychol.*

equal to the standard in density and the value at which it was judged equal in intensity was taken as a measure of the amount by which the two tones differed in density for the particular *O*.

The results are shown in Table I. Values are given in both volts and db. The values in db were computed from the

TABLE I

THE RESULTS OF MATCHING 6 TONES TO THE STANDARD FREQUENCY 500 CYCLES AT THE INTENSITY LEVEL OF 1.0 VOLT, OR 60 DB ABOVE THE THRESHOLD VALUE OF .001 VOLT

Each entry is the average of 10 observations.

Frequency	*V*		*N*		*Ne*		*Od*	
	Volts	db	Volts	db	Volts	db	Volts	db
Density:								
400	2.04	66.18	1.34	62.54	2.35	67.42	1.51	63.58
450	1.63	64.24	1.16	61.28	1.92	65.51	1.35	62.60
475	1.22	61.72	1.10	60.82	1.72	64.71	1.19	61.51
525	.82	58.38	.94	59.46	.99	59.99	.90	59.08
550	.69	56.78	1.01	60.08	.85	58.59	.85	58.59
600	.44	52.86	.99	59.99	.77	57.73	.75	57.50
Intensity:								
400	1.35	62.60	.94	59.46	1.53	63.69	1.12	60.98
450	1.18	61.42	.98	59.82	1.38	62.80	1.13	61.06
475	1.05	60.42	.98	59.82	1.34	62.54	1.07	60.58
525	.98	59.82	1.04	60.34	1.26	62.00	1.05	60.42
550	1.01	60.08	1.29	62.22	1.23	61.80	1.05	60.42
600	.87	58.79	1.38	63.80	1.25	61.94	1.05	60.42
Difference Between Density and Intensity in db								
400		3.58		3.08		3.73		2.60
450		2.82		1.46		2.71		1.54
475		1.40		1.00		2.17		.93
525		−1.44		− .88		−2.01		−1.34
550		−3.30		−2.14		−3.21		−1.83
600		−5.93		−3.81		−4.21		−2.92

liminal value which was empirically determined as .001 volts. It will be noted that the magnitude of the difference between the value at which a given tone was judged equal to the standard in density and the value at which it was judged equal in intensity is approximately the same for each *O*. One can thus average these differences for the purpose of plotting a contour representing equal density.

Table II shows the coefficients of variation (100 A.D./Mean) for the data of Table I. The average coefficient of variation of the judgments in terms of density is larger in the

TABLE II

COEFFICIENTS OF VARIATION OF THE RESULTS OBTAINED FROM 4 *O*s

Each coefficient is based upon 10 observations.

Frequency	*V*	*N*	*Ne*	*Od*
Density:				
400	15.25	8.06	7.53	8.01
450	13.87	6.90	9.01	8.74
475	12.13	4.27	10.52	7.48
525	9.39	6.81	10.51	5.56
550	14.64	8.22	12.24	5.53
600	8.41	10.20	7.79	12.80
Average	12.28	7.41	9.60	6.02
Intensity:				
400	11.40	9.04	6.99	6.61
450	9.49	6.84	4.49	5.31
475	7.24	5.61	6.42	4.58
525	5.82	5.38	7.62	4.95
550	8.51	5.43	8.94	5.90
600	7.14	8.18	7.44	7.43
Average	8.27	6.75	6.98	5.80

Average of the average coefficients for density 8.83
Average of the average coefficients for intensity 6.95

case of every *O* than the average coefficient of the intensitive judgments. This difference may be interpreted as meaning that the differential limen for density is larger than the differential limen for intensity. A similar relationship has been found to obtain for volume. A comparison of the coefficients of variation for the volumic judgments obtained in the author's experiment on tonal volume with the present coefficients for density reveals the fact that the coefficients are of comparable magnitude, an equivalence which suggests that Halverson's [6] technique of the determination of differential limens is not adequate as a means of showing the univocal status of volume as an attribute. Had Halverson's *O*s been judging density instead of volume, the magnitudes of the resultant limens would not necessarily have revealed the fact.

[6] H. M. Halverson, Tonal volume as a function of intensity, *Amer. J. Psychol.*, 1924, **35**, 360–367.

Density is established as a discriminable aspect of tones distinct from loudness and volume by the nature of the contour shown in Fig. 1. There, the curve is plotted from the *differences* between the values at which two tones are matched in density and the values at which they are matched in loudness, so that the *O*s can not have confused density with loudness. The fact that they have not confused density and

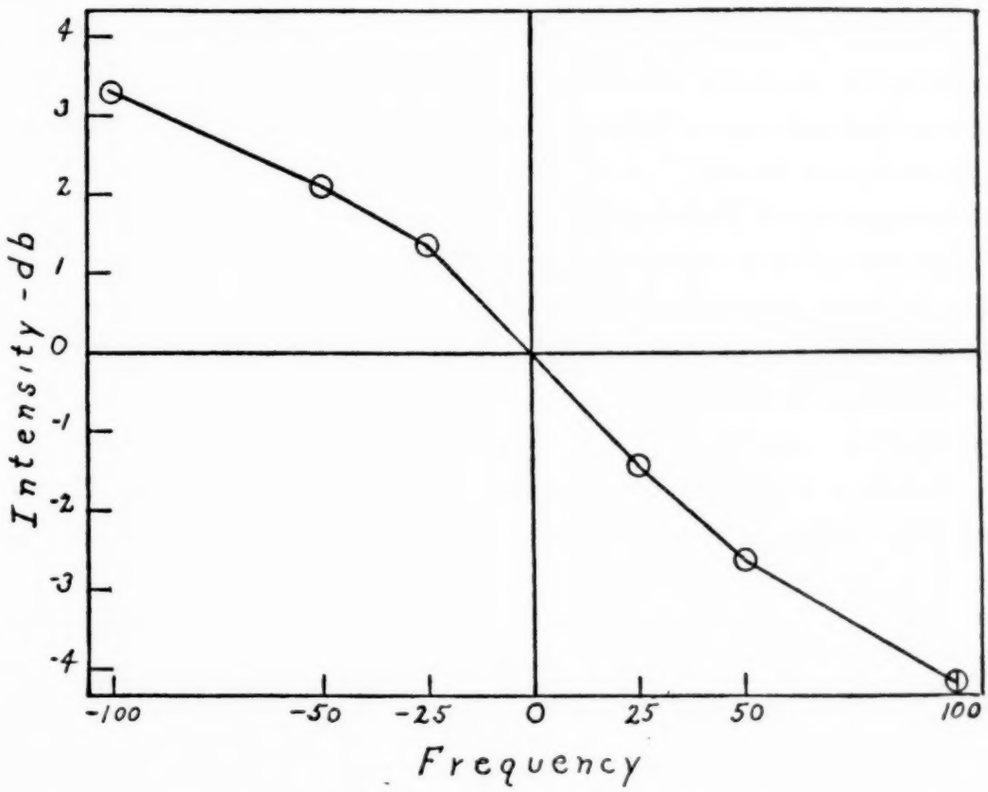

FIG. 1. Contour representing equal density of 6 tones equated to a standard tone (500 cycles, 60 db above the threshold), as plotted in positive and negative deviations of frequency and intensity from the standard, which is represented as the point 0,0 in the graph. Each point is the average of the data from 4 *O*s.

volume is apparent from the fact that this contour has a negative slope, whereas contour for equal volume, plotted in the same coördinates, has a positive slope. The function means that in order that two tones may be equal in volume, the higher tone must be made louder than the lower tone. To make them equal in density, however, the lower tone must be made louder than the higher.

The introspections of the *O*s show general agreement as to the phenomenal basis of their judgments. *V* and *Od*, how-

ever, stated that density was not 'penetratingness' or 'pierceingness.' They thought it might be possible to match two tones for 'piercingness,' but were uncertain as to whether the matches would be different from the equations they made for density. *V* also had some difficulty at first in getting the higher tones to lose their density when the loudness was decreased, a difficulty which tended, however, to disappear with practice. Towards the end of his series *V* said: "Low tones intrinsically lack density, but to make a high tone lose its density it must be made quite faint. Then it becomes much like a faint cloud. It loses its hard center, or rather, the hard center spreads out."

N stated: "The density of tones is not very hard to judge. It is one of the most direct judgments I have made. Density is very much the same as the penetrating quality of the tone. The thing I am judging as density is right in the middle of the tone; it is the core of the tone, whereas what I was judging as volume was the limits of the tone—the outer edge. Sometimes I get visual imagery. Then the low tones of poor density seem like attenuated smoke and the dense tones appear as solid, heavy smoke."

The report of *Ne* was: "Some tones are thick—others are thin or diffuse. The high tones are packed solidly. They are solid all the way through. If one of these tones is weakened in intensity, it becomes more scattered. Then it is thinner and less dense."

Od was confused at first by the difference in pitch between the two tones, but quickly overcame this difficulty. He reported: "Density seems to be somewhat analogous to saturation; that is, it is the ratio of the amount of tone to the amount of silence it is diluted in. It is the concentration of the tone—the amount per unit something or other. It is difficult to express."

The physiological basis of density is not obvious. One would like to think of it as dependent upon the density of the nervous impulses reaching the cortex via the auditory nerve. Volume, then, might depend upon the spread of these

impulses, as Boring [7] suggested, and intensity might depend upon the total number arriving per unit time. Such a situation is conceivable in terms of a resonance theory, but, in view of the present state of our knowledge, it is perhaps wiser to limit ourselves to some such statement as the following. The interaction of a sound wave with the auditory mechanism of a subject *who has been instructed to observe tonal density* sets up a dynamic pattern of neural excitation which is differentiated so as to issue in a specific discriminatory response. Since the response when the instruction is to judge density is different from the response which results when the subject is set to observe volume or intensity, it must follow that the total neural pattern is differentiated differently in each case, but the precise nature of the crucial neural pattern and the manner of its arousal by the cochlear mechanism remains obscure.

Another notion suggested by the outcome of this experiment is that the existence of density as a phenomenal dimension may account for the fact that *weak* tones are commonly referred to as *soft*. In the usual description given of a tone of slight density one finds such adjectives as 'soft,' 'penetrable,' 'loose,' 'diffuse;' whereas dense tones are called 'hard,' 'solid,' 'impenetrable.' For a given pitch, the dense tones are the loud tones; the tones which lack density are the weak tones. Hence, it may be that, in the absence of a convenient antonym for *loud*, the word *soft* came to be used, because it expresses one of the phenomenal characteristics of weak tones.

Summary

1. In several previous experiments density has been reported as one of the phenomenal aspects of tones.

2. The ease with which *O*s are able to equate in density two tones which differ in pitch by changing the intensity of one of the tones indicates that density is a discriminable aspect of tonal stimuli.

[7] E. G. Boring, Auditory theory with special reference to intensity, volume and localization, *Amer. J. Psychol.*, 1926, **37**, 167 f. The idea is more specifically stated by the same author in *Physical Dimensions of Consciousness*, 1933, 85.

3. The average coefficients of variation of the equation of two tones for density are larger than the coefficients of the equation of two tones for intensity, and the coefficients for density are approximately equal to the corresponding coefficients for volume.

4. The form of the contour for equal density, as plotted against frequency and intensity (Fig. 1), shows that the *O*s did not confuse density either with loudness or with volume.

5. The physiological basis of the experience of tonal density is at present obscure.

(Manuscript received June 14, 1933)

THE DEPENDENCE OF AUDITORY LOCALIZATION UPON PITCH.[1]

BY F. L. DIMMICK AND E. GAYLORD

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When Pratt offered the proposition that the designations 'high' and 'low' as applied to tones are based upon some fundamental spatial character of tones which varies with pitch,[2] he seemed to have settled a question which, as he pointed out, has been the source of much speculation. His solution affirms the very aspect of the problem which Stumpf, Wundt, and others had emphatically denied, namely, that "high tones are phenomenologically higher in space than low ones." Indeed, it was the apparently obvious negative of this which made necessary some explanation of the significance of the terms. Pratt submitted experimental data to substantiate his proposition. He presented to ten *O*s five pitches at the octaves from 256 d.v. to 4096 d.v. in five vertical positions and required them to locate the tones on a scale of fifteen points. The average vertical localizations of the tones followed exactly the order of their pitch numbers. When, however, we attempted to devise a simple demonstration based upon his results, we found ourselves in the difficulty of being unable to confirm them and, therefore, we undertook a more detailed experimental examination of the problem.

Procedure I

In our preliminary work upon which we hoped to base a demonstration experiment, we adopted the thesis that if "high tones are phenomenologically higher in space than low ones," tones of various pitches coming from directly in front of *O* will be separated spatially, provided, of course, that *O* is

[1] Presented at the meeting of the American Psychological Association at Ithaca in September 1932.

[2] C. C. Pratt, The spatial character of high and low tones, Jour. Exper. Psychol., 13, 1930, 278–285.

not influenced by knowledge of the position of the stimulus. For this experiment, therefore, *O* was seated in a dark room facing at 3 meters' distance a loud speaker which was actuated from an oscillator in the control room. *O* was shown a scale 2 m long of ten steps, and instructed 'to localize each tone according to the direction from which it seems to come by reference to its position on the scale.' We used the same five pitches as did Pratt.

TABLE I

AVERAGE LOCALIZATIONS ON A VERTICAL SCALE WITH 10 STEPS, OF TONES OF VARIOUS PITCHES GIVEN AT POSITION 5

	H	W	C
4096	9.6 ± 1.1	7.7 ± 2.6	6.2 ± 1.5
2048	8.4 ± 1.1	7.1 ± 1.4	6.4 ± 1.5
1024	6.7 ± 1.2	5.2 ± 2.2	5.8 ± 1.9
512	5.9 ± 1.1	5.4 ± 2.6	6.0 ± 1.5
256	4.9 ± 1.1	5.5 ± 1.7	6.1 ± 1.5

The results of these preliminaries as shown in Table I are equivocal. The judgments from only one *O* (H) resemble those given by Pratt; those from the other two show no consistent relation between pitch and localization. In the main, all tones tend to be localized above the middle point, 5, at which they were given.

PROCEDURE II

Since the 'spatial character' of tones failed to spread out vertically tones of various pitches given at the same position, as it might be expected to do and as it is said to do in music, we attempted to approximate Pratt's original conditions. We counterbalanced a small speaker over noiseless pulleys to move vertically through a distance of 3.3 m. A chair with a head rest was placed on a platform at a distance of 3.6 m with *O*'s ear on a level with the middle position of the speaker. The scale consisted of 5 equidistant points whose radii to the ear as a center subtended angles with the horizontal of − 25°, − 13°, 0°, + 13°, + 25°. Six pitches at the octaves from 128 d.v. to 4096 d.v. were furnished by a beat-

frequency oscillator whose output deviated only slightly from the true sine curve.

O was given the typed instructions, "You will hear tones of various pitches which will be sounded from 5 different positions on the vertical scale. Indicate by reference to the numbers on the scale the direction from which each tone seems to come." Also, he was shown the moveable speaker and the positions from which the tones might come in order to emphasize for him the localization character of his task. He was then blindfolded and the tones presented in haphazard order with respect both to position and to pitch. Five series of 30 judgments were taken from 4 *O*s with the tones presented in the front median plane, 5 series in the back median plane, and 5 series at the right, 90° to the median plane.

Results

Our results from this more carefully controlled work are in positive disagreement with Pratt's. Table II shows the average localizations of the several pitches by the 4 *O*s.

TABLE II

Averages of 75 Localizations on a Scale of 5 of Each of 6 Pitches

	C	D	M	S
4096.....	2.80 ± 1.12	3.36 ± .65	2.79 ± .88	2.88 ± 1.12
2048.....	2.43 ± .99	3.53 ± .69	2.32 ± .92	3.07 ± 1.0
1024.....	2.37 ± .94	3.40 ± .68	2.52 ± 1.08	2.52 ± .94
512.....	2.83 ± .84	3.33 ± .77	2.33 ± 1.2	2.73 ± .79
256.....	3.15 ± .90	3.28 ± .85	3.7 ± 1.07	3.35 ± .99
128.....	3.18 ± 1.03	3.08 ± .85	3.85 ± .96	3.55 ± 1.0

By no method of analysis could our results be made to indicate a relation between pitch and apparent position. For only one *O* (D) does localization parallel pitch with any consistency and his extreme spread is less than .5 of a scale division, about half of his M.V. Furthermore, this *O* was the only one who was aware of the significance of the results and of the possible relation between pitch and location. In the other cases the 'lowest' tones are localized higher than the 'highest' tones. In the main the *O*s tend to place tones at,

or just above, the horizontal so that the average localizations lie close to the value 3. The M.V.s are relatively large, however, and every average with its M.V. covers most of the mid-range of the scale, *i.e.*, 2–4. This middle tendency of the average localizations is due to two factors in the individual reports. In the first place, there was some tendency to avoid positions 1 and 5, as shown in Table III. In addition there

TABLE III

PERCENT OF LOCALIZATIONS IN EACH VERTICAL POSITION

O	C	D	M	S
Position 1	16	2	22	12
2	28	15	18	23
3	23	39	22	29
4	24	35	21	21
5	9	9	17	15

was an approximate balance of plus and minus errors; that is, any tone irrespective of pitch is as likely to be placed above its true position as below. 'Low' tones presented down were localized up and 'high' tones presented up were localized down as frequently as conversely. The correct localizations approximated 20 percent which is in substantial agreement with Seashore's results on localization in the median plane.[3] The horizontal position of the tone, front, back, or side, had no noticeable effect on its apparent vertical localization.

At the end of every series we asked our *O*s to tell how they arrived at their judgments of location. All agreed that they had no satisfactory criteria of the vertical position of the tone, that to all intents and purposes the judgments were mere guesses. One *O* (M) remarked that he would have liked to take the 'high' and 'low' pitch of a tone and apply it to his judgment of position, but that he realized that in so doing he would not be localizing the tone, because a 'low' tone often seemed nearer the ceiling than a 'high' one. The other *O*s were accustomed to the terms 'bright' and 'dull' for pitch differences and did not use 'high' and 'low' either for pitch

[3] C. E. Seashore, Localization of sound in the median plane, *Univ. of Iowa Studies in Psychology*, 1899, II, p. 46.

or for position. They spoke, instead, of the position of the tone as 'up' or 'down.'

Conclusions

Our results, then, taken under conditions which we tried to make equivalent to Pratt's, contain no evidence that 'highness' and 'lowness' as applied to tones have a spatial significance, and deny his contention that "high tones are phenomenologically higher in space than low ones." The determinants of the divergent results must lie in the *O*'s *Aufgabe*. There may have been essential differences on the one hand in the instructions given to *O* either verbally or by the obvious experimental setting, and on the other, in the understanding of the problem and in the attitude taken by the *O*s observing in the two experiments. Pratt does not quote his instructions, but states that "observers were asked to locate on a numbered scale running from floor to ceiling the position of tones." In addition, "the observers were allowed to know that the receiver was being placed at different points up and down in the vertical scale." In these respects it does not appear that the two sets of conditions differed widely. However, we laid more emphasis on the localization aspect of *O*'s task than is evident in the bare statement of the instructions. These were re-presented at every rest period during the observation hour. *O* was allowed to see the speaker changed incidentally (but not in the course of the observations) from one position to another until it was evident from his reports that he had no doubt about the actual positional changes of the stimuli, or of our reasonableness in expecting him to indicate those positions. We adopted this procedure to preclude, if possible, the confusion of pitch-quality with either spatial or pitch localization. The results justify it. Spatial localization was difficult and inaccurate but pitch-quality certainly was not substituted as a secondary criterion of position. 'Pitch-location' did not intrude to modify the space judgments, either by raising high pitched tones presented down and conversely, or by increasing the correct judgments of 'high' tones presented up and 'low' tones presented down.

The *Aufgabe* that depends upon an *O*'s general and psychological background and specific training is, of course, difficult to evaluate, but in this case must form the major factor in the disagreement of results. Pratt tells us nothing about his *O*s and it would be absurd to speculate. The question is not in any sense one of better or worse observation. Three of our *O*s were free from any theoretical basis that could have affected the results. They were undergraduates who had had two years of laboratory training but who were unaware of the nature of the problem. One man was musically trained. The fourth *O*, on the contrary, is the senior author and as such planned and closely supervised the experiment. It is significant that his results somewhat resemble those of Pratt's *O*s.

Pratt states that "only at the outset did the observers experience any difficulty with the judgment. The tonal impression seemed at first to pervade the whole room, but as the attentional direction fell in line with the task imposed by the instructions this difficulty entirely vanished and the judgments were made easily and quickly, and with surprising consistency." Our *O*s always found the judgments difficult, the tone directionless, although in addition to several preliminary series every *O* made 75 judgments on 6 pitches, a total of 450 judgments against 10 judgments on 5 pitches or 50 judgments in all by each of Pratt's *O*s. Obviously their judgments of 'pitch-localization' were based upon a different perception from that of spatial localization and consequently do not demonstrate more than a figurative significance of the spatial terms as applied to tones. Pratt's statement that "as soon as one has his attention directed to the spatial property of a tone the phenomenon of pitch locality becomes very real and unmistakable" is a further indication that his spatial judgments do not reflect an ultimate spatial dimension of the tones but are based on perceptions of a somewhat higher order in which the spatial aspect has become figurative.

If 'pitch-location,' then, is a perceptual judgment, one that readily becomes implicit as so many perceptions do, but as such does not imply an ultimate dimension or attribute of

tone, its implications for the phenomenon of apparent auditory movement are somewhat different from those Pratt suggests. Musical 'movement' differs fundamentally from visual or cutaneous movement in as much as it is based not upon an attribute of extent but upon a spatial perception in which the implication of extent is figurative. Since, however, this figurative spatial perception so readily becomes ingrained or implicit and seems to *O*s to be immediately spatial, it is natural that musicians should easily acquire the perception of pitch-locality and hear temporal shifts of tone quality in music as 'movements.' It follows that such musical 'movements' would not be perceived by the musically untrained. Such, indeed, has been my own casual observation. To the uninitiated the term movement as applied to music is almost, if not quite, unintelligible.

(Manuscript received April 15, 1933)

THE CYCLOPEAN POINT IN TOUCH

BY PROF. K. C. MOOKHERJEE

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An attempt has been made in this experiment to determine the nature of localisation of the fused impression of touch. The subject was to localise the fused impression that arose when the two points on the different parts of his left hand were touched by an aesthesiometer. The distance between the two points of contact was different in different parts and was such as was necessary for the fusion of impression.

Ten senior students were chosen for the experiment. The volar side of their left hand from the tip of the middle finger up to the elbow joint was explored in different parts. The middle finger, the palm and the fore-arm were each longitudinally divided into three parts, and each part was examined. Their lower lip, the tip of their nose and the middle part of their forehead were also tested to observe the influence of the delicacy of the points on such localisation. Screen, felt, Spearman's Aesthesiometer etc. were used and all necessary precautions taken.

Sensibility of the nine subjects out of ten was normal and their localisation of impression was also somewhat of the same nature. It is one subject only who showed exceptionally low sensibility and was treated separately from others. In treating the results of the other nine subjects we find that the variation of distance between the two points of contact ranges between 1 mm and 8 mm in the three parts of their middle fingers, between 4 mm and 11 mm in the three parts of their palms and between 6 mm and 15 mm in the three parts of their fore arms.

When two points at the tip of the middle finger were stimulated the fused impression was localised by all subjects except one to a point lying between the two points of contact. Other localisations, *i.e.* at the middle part and at the base of

the middle finger, were more or less similar to it. So out of twenty-seven localisations that were altogether made by nine subjects in three different parts of the middle finger there were only four cases in which the point of localisation was found deviating slightly from the middle point between the two points of contact. At the palm of the hand nine localisations were made exactly to the middle point and of the remaining eighteen localisations that were made more or less irregularly it is only in four cases that the irregularity occurred most and the point of localisation departed there from the middle point by 17 mm, 26 mm, 7 mm and 11 mm. While in eleven cases the departure is very slight and in other three cases the point of localisation coincided with the left or the right point of contact. In fact here also twenty out of twenty seven localisations were made to the middle point or nearby. In the forearm ten localisations were made definitely to the middle point, while seven were kept away from it by 3 mm, six by 4 mm or so and the remaining three only were very irregular. Here, too we find localisation was mostly made to the middle point or to the points near about it. At the tip of the nose and at the middle parts of the lower lip and of the forehead the fused impression was localised by all subjects definitely to the middle points.

These results point out that the localisations of this kind goes together with the delicacy of the skin. At the highly sensitive parts of the skin such as the tips of the finger, of the lips, of the nose, etc., localisation was made in all cases—almost definitely to the middle position of the two points of contact. In other parts of the skin localisation deviated from the middle part much with the deminuation of its sensibility. This truth has also been negatively found out in the case of the subject whose sensitivity of the skin is comparatively dull and who has therefore been considered separately. With this subject the distance between the two points of contact ranges between 2 mm and 13 mm, between 17 mm and 18 mm, and between 22 mm and 30 mm in the three parts of his middle finger, of his palm and of his forearm respectively. Of the nine localisations made by this subject three were ap-

proximately made to the middle points and the remaining six to some distant points which lay in two cases about 33 mm away from the middle point. It is a fact here to note that the irregularity of localisation goes with the increasing insensibility of the skin. At the middle part of the forehead and at the tips of the nose and of the lip wherein the sensibility is much higher the localisation was also made much approximately to the middle point.

It is to be generally observed that when two impressions of contact get fused without any discrimination the localisation of the fused impression is made to the middle part of the two points of contact or to other irregular point as the part stimulated is delicate or not. So localisation of the fused impression to the middle point or near about it is to a great extent in relation to the delicacy of the skin stimulated. This mode of localisation is akin to the cyclopean character of vision and occurs with the most discriminative parts of the skin.

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